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Official British Photos

Testing telephone lines at a field service station

BATTLE TELEPHONES [See page 376]

# Ordnance on the Allied Front—I*

## Developments in Artillery During the War

By John Headlam, Major-General, in Charge of the British Artillery Mission

If I can not claim to be one of those artillerymen who have invaded the domains of science, I have learned, as an artillery commander in the field, how much we owe to the scientists who have devoted their talents to the solution of the problems which confront us. I think therefore that I may be able to tell you something of the development of artillery during this war, from the user's point of view, which may be of interest. The subject is so vast that it would be quite impossible even to touch on, much less to deal adequately with all the directions in which these developments have take place, even if I were to devote myself entirely to the science of artillery.

But this is not everything. To engage the right target from the right place, with the right guns, this is the artilleryman's *art*, without which his *science* is half wasted. In artillery, more than in any arm of the service, tactical and technical considerations are most intimately connected. I shall try to show how the changes in tactics affect technical matters and how the demands of the soldier may upset the plans of the scientist and I hope that I shall be able to make you understand that the entry of science into war has in no way taken away the glamour of romance, however much it may have increased its horror.

I shall not trouble you with any reference to organization. You must accept it from me that every operation in this war has confirmed the necessity for a carefully elaborated scheme for the employment of concentrations of artillery, involving a methodical allotment of tasks from the outset, and largely depending for its successful execution on an effective chain of artillery command.

### FIELD ARTILLERY MATERIAL

It will, I think, be advisable to commence with some brief reference to the various types of artillery employed in the field, without attempting to go into detail regarding any particular nature. Taking *field* artillery first, we stand practically where we were. At one time an idea got abroad that the days of field artillery were numbered, that it had been supplanted by the "heavies." This was a very dangerous heresy, but it is dead. The field artillery is as firmly established in its position as it ever was, the proportion of guns to bayonets has altered little, and the material has changed least of all. The Germans still have their old field gun, made as a breech loader in 1896 and converted into a quick firer in 1906. Our own 18-pounder, which was brought into the service at the beginning of this century, immediately after the South African War, is still in our belief admirably suited for its work. It is no secret that the American artillery has adopted a field gun designed and made by those admirable gunners, the French, in the nineties of the last century.

### SHRAPNEL VERSUS HIGH EXPLOSIVE

But there are two points in connection with field artillery material on which a real difference in opinion and practice exists.

The first is the relative values of shrapnel and high explosive shell. On this the French and ourselves are the great protagonists of the rival schools. Their 75-mm. is primarily designed for the rapid fire of high explosive shell and its "rafales" did marvels in throwing back the first great German advance on the Marne. The English, as befits the country of the inventor, General Shrapnel of the British Army, have always been great shrapnel adherents; our 18-pounder was designed as a shrapnel gun, and has undoubtedly the most powerful shrapnel in existence. I admit that we carried this too far in having no high explosive at all, and you may possibly recall the great outburst of indignation caused by some so-called "revelations" in the press regarding the shortage of high explosive shell in 1915. The question now is not of there being enough high explosive, but of getting the artillery to use the proportion that the manufacturers would like to produce. We saw the effect of our shrapnel on the German infantry in 1914, and we have not forgotten it.

The second is the advisability of including howitzers in the field artillery. Here again we join issue with our French friends, who hold that the lowest caliber for the howitzer is the 6-inch. But we know what our howitzers have done for us, and as I see again in memory the many fields on which our 4.5-inch rendered such yeoman service, ever since they first came into action among the slag banks and pitheads at Mons, I can scarcely imagine how we should have fared without them. And here I know that our infantry would back me up.

### USE OF HEAVY ARTILLERY IN THE FIELD

When we turn to heavy artillery, in which I include all natures other than field, the story is one of almost miraculous progress. Broadly speaking, it may be said that heavy artillery formed no part of the equipment of modern armies at the commencement of the war. This is not strictly true, for the Germans had a battalion of 6-inch howitzers in each corps, while we had a battery of 60-pounders in each division. But the use of heavy artillery in the field was no new thing, though the whole world gasped in amazement when the Germans brought up their big siege pieces on the Aisne. In modern days it had been done with success by the Boers, who used the 6-inch guns taken from the fortifications of Pretoria with success against us on many occasions. As soon as Port Arthur fell, the Japanese brought their 11-inch howitzers to join the field army in Manchuria, and the mountings of some of your coast defense guns on their way to join the field army in France are modeled on those which our sailors improvised to carry their ship guns across the South African veldt.

### PROPORTION OF HOWITZERS

In field artillery we have seen that controversy raged around the inclusion or not of howitzers at all. In heavy artillery all are agreed that both are necessary, but opinion is at variance as to the proportions of each. Here again we find ourselves on the side of the Germans while the French are, or were, all for the gun. But in this matter as in so many others, friendly discussion, and the exchange of experience, has brought us very much to the same point of view. The whole thing really turns on whether howitzers should be used for counter battery work or confined to bombardment. We hold that the howitzer is more accurate against such a target as an enemy's battery; that it is more mobile for the same weight of shell; that its fire can be more easily observed, especially from an aeroplane; that it has a longer life; and that it is easier to place, not only because it can be tucked away into hollows of the ground where it would be impossible to use a gun; but also (and this is a curious development of this war of masses), because it can be used in crowded areas. I have often seen our big howitzers in action in the midst of a mass of congested traffic around some depot of supplies, with troops moving in every direction, wagons loading with up stores, and so forth, under their very muzzles, where the blast of a gun would have swept everything away in front of it. But while controversialists are marshalling such arguments it looks as if the ground would be cut away from under their feet by the disappearance of the distinction between gun and howitzer. The length of the howitzers has gradually grown from 13 calibers to 17, and now in some of the latest to over 20, while the use of varying charges either to reduce the wear, to or allow of the employment of curved fire, is gaining ground for guns.

### TRENCH MORTARS

At the other end of the scale we have trench mortars, again only a revival of a weapon well known a century or more ago. They also were used extensively by the Japanese in Manchuria, but I am afraid that the artillery as a whole was rather inclined to scoff at them when they first appeared. Those days, however, soon passed. It was recognized that in the form of trench mortars, we could increase our strength for bombardment by utilizing *material and labor not good enough for guns*. The German trench mortars, for instance, were chiefly made by a firm famous the world over for its cream separators. What distinguished officers at one time called "tin pot artillery" thus rose gradually in favor, until in Italy it took the place of horse artillery as a *corps d'élite*. But in those days we were shorter of guns and ammunition than of men. Now the position is reversed. Trench mortars are terribly expensive in men, and more especially in officers, and so just when the material is being perfected, the mortars are dropping to some extent out of use. It is just one of those changes which must be so baffling to the civilian who tries to help us.

### INCREASE IN RANGE

With all natures, guns and howitzers, field, heavy, and trench mortars, there has been a continual cry for range, and still greater range. This has been due to many causes, chiefly tactical, but principally to the extension of field of fire given by aeroplane observation. The various expedients which have been resorted to to meet this demand are insteresting. In the first place there was the simple lengthening of the howitzers, and the bringing into the field of the long high-velocity naval and coast-defense guns. Then there came the various alterations in form of shell, the general feature of all of which is a great lengthening of the ogive (the pointed nose of the projectile). The radius of curvature of the ogive has been increased from 2 to 6, and in some cases to 8 calibers, but the most notable form is the French "Obus D," so called after General Desailleux, the officer chiefly responsible for the design, and usually designated by us as the "streamline" form. Our experience with this form has not been very encouraging, but it is admitted that the slope of the base has a very important bearing, and must be determined for each nature, and I may mention as an instance of the close cooperation existing between us, that the French have recently designed a shell for trial in our guns with which we hope to get better results. The "false head," another form of the same idea, is largely used by French and Germans in the heavier long range guns, but again it is not so popular with us. It introduces considerable complication in supply and fitting and is suspected of being a possible cause of "prematures." No doubt with the extension of the use of longer headed shell, the necessity for false heads disappears to a great extent. But I am by no means satisfied that they have not great possiblities when used with shell designed from the commencement for them.

### LONG-BURNING FUSES

The most difficult problem in connection with range is the provision of a time fuse which will be reliable for long range fire. We should use shrapnel far more than we do, if we could get a good time fuse, reliable at long ranges. Here is an opportunity if ever there was one for the scientists. As you probably know, the fuses in the service now depend on the burning of a train of composition, which is liable to many inaccuracies, especially when you consider the conditions under which it has to be kept on service. Mechanical fuses have been known for long, and we did our best to encourage inventors nearly twenty years ago, but such fuses only came into real use last year when it was noticed that the Germans were making uncommonly accurate practice at our balloons, at ranges up to over 20,000 yards. Early in the summer we got specimens of their fuse, which turned out to be a clock-work fuse designed in 1916. The Academy of Sciences will be interested to hear that they were at once handed over to the Cambridge Physical Laboratory—it would perhaps be indiscreet to proceed further with my revelations.

### USE OF GUNS AT CLOSE QUARTERS

But if the tendency is always to increase the range of our guns, do not think it is with the object of keeping them back. Far from it, for even if guns are as a rule further apart than they were in the old wars, the men who direct the guns are closer than they were even in Napoleon's time, and there is still as much room as ever for the display of personal enterprise and gallantry. But as a matter of fact, just as this war has seen the revival of hand-to-hand fighting with the bayonet and the butt, so it has seen guns pushed into closer ranges than has occurred for a century at least. On many occasions I have known individual field guns put within 200 yards of the enemy's trenches. This was of course for some special task, such as breaching a parapet or knocking out some particularly obnoxious "nest" of machine guns. With time, ingenuity, and courage, a gun can be got almost anywhere, and the effect of its fire at such ranges is very marked, while its presence affords immense encouragement to the infantry. Great care must of course be taken in working out the preliminary arrangement, and in one case I may mention where a gun had to be brought up over the open, it was moved at night under a canopy, like a dignitary of the Church in high festivals, and the gunners who carried the canopy were trained to drop it on the gun whenever a flame went up. This gun fired its 100 rounds at a range of 70 yards in nine minutes, completely destroying its objective, and the detachment then, strictly against orders, joined in the assault.

Another case I know forms rather a touching story. When I was on the Italian front beyond Gorizia in 1916 I happened one day to see a gun very cleverly concealed in the front line, to be used in much the same way.

*A lecture given before the Washington Academy of Sciences on April 3d, 1918. From the *Journal of the Washington Academy of Sciences*. Communicated by Mr. L. J. Briggs.

Curiously enough I met last year the commander of the corps to which this gun belonged, and talking one day he asked me if I remembered it. He said he had been going around after me, and the noncommissioned officer in charge had told him how an English general had shaken his hand and congratulated him on being in the place of honor. Poor fellow, he did his work with complete success next day, but he and all his men were killed.

#### INCREASE OF HEAVY ARTILLERY

But, gentlemen, I am not sure that the real romance of artillery in this war does not lie in the efforts made to furnish us with the material we so urgently needed. At the beginning, as I have said, we had one battery of "heavies" per division, or 24 guns in the whole of our "contemptible little army." On the Aisne we got our first siege howitzers of 6-inch caliber, and I had placed under my command there the same battery which as a young staff officer I had guided to its first position against the Boers at Pardeberg. During the winter of 1914, a few more heavy guns and howitzers began to arrive, but by midsummer of 1915, we had only about 70 all told. The summer of 1916, however, saw this number increased just tenfold, while by last summer it had been more than doubled again. How was this done? In the first place by utilizing every gun, whether designed for a fort or a ship, that we could lay hands on. The mounting of such guns, for work in the field, either on railway trucks or carriages, has given great scope for ingnuity, especially as the task has become more and more complicated by the necessity for economy in metals and in skilled labor. But all along the great consideration has been *time*, and this of course has been especially true of new manufacture. It is to that element of time that I would like to draw your special attention, because it is one which, if you will pardon me for saying so, the scientist is perhaps a little inclined to overlook. It is only natural that he should be absorbed in the perfecting of his design, but the poor soldier facing the German can not wait for the fairy tales of science and the long results of time, but wants something, anything, and quickly, that will shoot.

#### NATIONAL EFFORTS

And then Mr. Lloyd George, like a new Peter the Hermit, led a crusade to stir up the people at large to the manufacture of guns and shells. We perpetuated designs which we knew to be out of date. We adopted, with our eyes open, new designs which were in many points based on considerations of facility of manufacture, rather than of perfection, and we risked the omission of many of the regular stages of trial hitherto considered essential. It was a gamble, but it was the only way to get the numbers required, and it was justified by success. In this connection I may perhaps mention a most remarkable instance of adherence to antiquated pattern, in order to avoid any delay to output, afforded by the Germans. The outbreak of war found Germany, as I have already mentioned, with an obsolescent field gun, but as I personally discovered in the battle of the Somme, she directed all her energies, not to remedying its defects, but to developing production. I happened by accident to examine there two captured guns which were standing side by side. One, No. 40, had been made 20 years before and converted from breech loading to quick firing; the other, No. 6173, had only been made the previous year, but there was not a rivet's difference between them; only, in the new gun, time had been saved by omitting engraving the Imperial cipher on the breech.

#### AMMUNITION

As with guns, so with ammunition, but perhaps, to a still greater extent, production has been the great problem, for from early in the first autumn of the war our stocks of ammunition were practically exhausted, and we gunners had over and over to turn a deaf ear to the calls for help from our almost exhausted infantry. Everything possible was done to expedite output. National shell factories were set up all over the country, for the smallest shop could at least make 18-pounder shrapnel bodies, and delicate women toiled long hours at the lathes. We adopted designs which were not the best but which were the easiest to make, and then faced the danger of "prematures."

This bursting of guns by the premature explosion of the shells is almost inevitable when one has to depend on hurried and unskilled production: it is one of the risks which must be run when shells have to be rushed out to the front. But the loss of guns and men may be serious, and it is always a trying ordeal to the artillery. The French with their large expenditure of high explosive shell were the first to suffer severely from it. I remember seeing many of their wrecked 75's when we were fighting side by side at Ypres at the time of the first gas attack, but they bore it with the calm fortitude which has been their attitude through all these long years of trial, and when our own time came, their experts placed all their experience at our disposal, and rendered us invaluable assistance in getting through our trials, and I would like especially to mention here the names of General Gossot, an artilleryman who has gained more than a national reputation as a contributor to science, and General St. Claire Deville, whose name is a household world in France as "the father of the seventy-five." It will do no harm now, and may do good, to tell you how serious our position was at one time. [The ratio of prematures, at first irregular, then rapidly decreasing, was shown by tables.]

There will always be prematures, and loss of life from them, while high explosive shells are used, but we look to science to apply its methods to the investigation of every case, and to guard us, as far as human ingenuity can, against them.

But what did we gain by accepting these risks? The average number of tons of ammunition fired away per week in France will probably be the simplest way of putting it. [The figures, in tons per week, showed the immense increase in output attained since the war began.]

#### ECONOMY OF MATERIALS

It was not until our production was assured that we were able to set ourselves to improving our designs, and then came the necessity of economizing materials, to dampen the enthusiasm of our designers again. We have had to reduce the capacity of our favorite 18-pounder shrapnel to allow of the use of lower grade steel. We have had to replace our well tried propellant, cordite, by nitro-cellulose; to reduce the percentage of T.N.T. in our explosives; to let brass displace aluminum, and cast iron displace brass, in our fuses; and to change the form of our driving bands to economize copper. But everywhere again science has come to our aid once the need has been fairly put.

#### PRODUCTION

Judging from our experience, the guiding rules in order to insure production would appear to be to develop to the utmost the production of what can be got easiest—remembering always that there will certainly be a demand for changes, and to press on research in the meantime so as to be ready to change to more efficient patterns as soon as the position allows of it, watching always the tactical changes so as to be able to anticipate demands. Thus the business man and the scientist have full fields for their activity, but both will have many discouragements to face, for in war they must be controlled by the needs of the soldier.

When the scientist after weeks of intense study has solved the secret of some wonderful idea for improvement in design, he will be told that it is not worth the loss of output it will entail; for to every change—however fascinating or desirable in itself—must be applied the touchstone: "How much will it retard output?" Just when the manufacturer is priding himself upon the introduction of improvements in method which will shortly double his output, he will be told to shut down.

So it is, and so it must be—*war is not, and never will be, a business proposition.*

#### WEAR OF GUNS

The output of new guns has not only to provide the numbers required to bring the army up to the desired strength, but it has to meet the wastage due to accident, to the enemy's fire, and to wear, of which the last completely overshadows the other two.

As long ago as 1916, General Gossot said in my hearing, "Up to this the guns have eaten up shells; we shall now see the shells eat the guns." He was absolutely right. At the beginning we had little anxiety, for so admirable was the material of which our guns were made that their lives proved in practice to be far longer than had ever been anticipated. But as the output of ammunition increased they began to give out, and it may interest you to have some figures as to what the "lives" of the more important natures are. [Tables were presented showing the average life of guns and howitzers.]

Needless to say that the search for a cure has been pursued with vigor, and this is a matter in which there is a great field for science; a field which has not been overlooked in this country, as witness the learned paper on the subject by Dr. Howe in the *Transactions of the American Institute of Mining Engineers* of last February. There is no doubt that the intense heat caused by prolonged rapid fire has brought on the guns a strain which was never anticipated, and in France and Russia and Italy I found that deterioration in the quality of the steel used since the war began was thought to have been a contributing cause. With us this latter does not appear to have been the case, except perhaps, in individual instances, nor have we been able to determine whether carbon, nickel, or nickel-chrome steel gives the best results.

Reduced charges have now been introduced. Strict rules as to pauses to cool the guns have been promulgated, and various substances are now used for greasing the bore. We hope to get good results from our latest mixture, the composition of which has, needless to say, been communicated to your ordnance authorities.

#### REPAIR OF GUNS

But all the above are merely palliatives. We have to face the fact that our guns have all a very limited life under modern conditions. One battle may be enough for some, and so the question of *repair* has become one of great and growing importance. Facility for relining must ever be in the mind of the designer, the provision of sufficient plant for repair must be included in the outfit for war, and a regular system of withdrawal in rotation instituted. Just as in a fleet some ships must always be in dock if the docks are not to be congested by a sudden rush, so must a regular system of sending guns for relining be maintained.

With the ocean between your guns and your arsenals, the problem is a very difficult one for you.

#### WEAR OF CARRIAGES

And the same thing applies to the carriages. The delicate mechanism which is an essential feature of a modern carriage, even in field artillery, requires skilled and careful handling, especially when called upon for such a strain as is imposed by long continued rapid fire, with its consequent heating of all the parts, expansion of oil in the buffers, and so forth. It has always been the boast of artillery officers to know and care for their equipments, but the entry into action of large bodies of newly-raised artillery in 1916, synchronizing as it did with the enormous development in ammunition supply, undoubtedly led to a considerable amount of preventable damage. Where this struck us particularly was in the springs of our field carriages, and in the air recuperators of some of our heavier mountings. One divisional artillery commander told me in August that his guns had fired 7,500 rounds in six weeks, and that since the beginning of the action he had had on an average 25 per cent of his guns always out of action from this cause. All the spring-makers in England were called into conclave—representatives of the design departments of all the great gun making firms were taken over to France, to see on the spot where the failures were. But no doubt the chief damage was due to the fact that in the heat of battle, inexperienced personnel had forgotten the constant attention buffers require. Great attention has since been paid to this part of the training, and after visiting many of the field workshops a few days after the commencement of the attack in Flanders, I was able to report that preventable damage was practically dead. But there are still, alas, some cases of prematures, and with the counter battery work that goes on now many cases of damage from the enemy's fire, so that our field workshops are still kept busy. Close up to the front you will find everywhere installed in ruined farms or under a tarpaulin shelter these ordnance workshops, containing a heterogeneous collection of damaged guns and carriages. From the store of "spares" it may be possible to put the damage right with some adjustment, or from three such "lame ducks" it may be possible for one or two to be made complete, and so the work goes on all night, and by dawn the guns are in their places in the line again. The work done by the officers and men who man these workshops is a very material factor in the great artillery struggle, but nothing can compensate for the daily care of the gunners, and I always think the mottoes inscribed on the French 155-mm. Filloux guns should be on every gunner's heart:

"Le Canon bien tendu en vaut deux." "Soyez bons pour les freins."

I hope, gentlemen, that you will not think I have devoted too much of your time to this subject of production and repair, but it is one of absolutely vital importance to the efficiency of an army in the field, and it is one in which science has a great part to play.

#### ACCURACY OF FIRE

I had intended to tell you something of the development of the work of artillery in the field, of counter battery work, and of the "barrage," a word which seems to have captured the American imagination almost as much as "camouflage." But time does not permit, so I will confine myself as far as work in the field is concerned to giving you an idea of what has been done in the way of developing the *accuracy* of artillery fire during the war.

Accuracy of fire is of course the first essential to success in the artillery, and the first thing therefore that the good gunner does is to get as good a platform for his gun as he possibly can, and that probably means much heavy labor in digging deep, and gathering material—rubble, bricks, timber—from a distance. This search for materials sometimes leads to amusing scenes.

[TO BE CONTINUED]

### A Mysterious Fish with Arabic Inscriptions

A STRANGE fish was recently caught at Zanzibar with Arabic characters upon its tail. The fish was not one of a large haul, but was caught by a single fisherman, who brought it to the fishmarket. There it remained for some time, having no purchaser as it was a strange fish and one that had never been seen before in those waters. Finally, an Indian of the sect called "Memen" purchased it, and on the strange markings being noticed, it was taken to a well-known Arab scholar; who deciphered the inscription. It was afterwards taken to the Sultan who also recognized the wording. That night 3,000 rupees was offered for the fish and refused, and on the following day 5,000 rupees was refused. The original price paid for the fish was five pice (about three cents), and it was eventually decided to have it preserved. For this purpose it was taken to the Government Laboratory where it was treated with formaline. It has since been placed on public exhibition. There are two distinct inscription on the tail, one on each side: One reads: on one side "Laillaha Illalah"—"There is no God but Allah"—and on the other "Shani Allah"—"A warning sent from Allah." There is no suspicion of anything in the nature of a fake about the matter, and the mystery is so complete that no explanation of the strange phenomenon is forthcoming. The Arabic lettering is perfectly plain and the discovery has caused wonderment throughout the Mohammedan community of Zanzibar. It was identified as *Holacanthus semicirculatus*, Cuv. et Val., a widely distributed Indo-Pacific species of Chætodontidæ. An authority on zoology considers the markings as falling within the limits of normal variation of the species.

### Migratory Butterflies

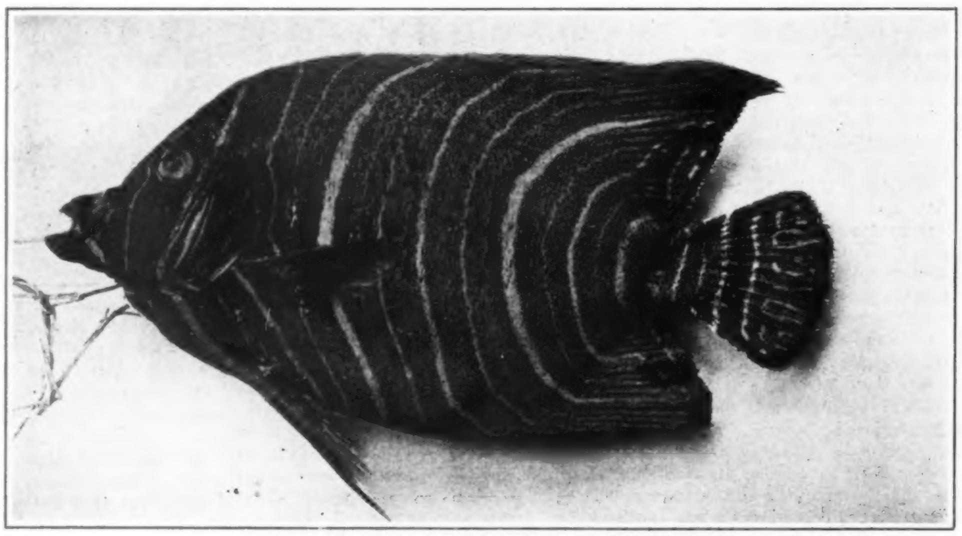
IT is generally known that many birds make migrations annually, and fairly well-known that certain fish make similar migrations, but even entomologists are not always familiar with the fact that there are migratory insects as well. Of these perhaps the most striking case is that of the the monarch butterfly the large brown and black butterfly so common to the United States. In the hall of insect biology in the American Museum of Natural History has been installed a most interesting group showing hundreds of monarchs swarming on a small white oak, in early autumn, preparatory to making their migration south. They have settled on the tree in thick clouds, clinging everywhere to branch and twig.

During the summer the larvae of this species feed on milkweed. They are protected from insect-eating birds by their "warning colors," which proclaim them not good to eat. The adults, which emerge in the fall from beautiful green chrysalids also have a gaudy coloring which indicated that they are inedible. With the coming of fall the monarch butterflies begin their movement southward, thousands flying together, and stopping from time to time at certain resting or gathering places, which, strangely, seem to be used year after year. In such places great swarms of the monarchs settle down on the trees, sometimes almost completely disguising their foliage, their brown wings so like leaves giving the trees a late autumn appearance. It was from one of these swarming places near Clinton, Conn., that the specimens for a Museum group were obtained.

When the clouds of migrating monarchs, having reached something like their full strength of number, continue their southerly flight, the air is filled with the soft beating of their brown wings, which gleam with a ruddy light in the sunshine. An article by Howard J. Shannon, written for *Harper's Magazine*, describes such a migration: "They travel parallel with the ocean, and in irregularly regular order—that is, at fairly even distances apart and so concerted is the movement that even my sudden striking gesture with the net turns aside only the insects immediately attacked; it does not disturb the onsweeping advance of the general body that seems like a sentient river in irresistible, ceaseless flow. Indeed, their number is enormous. For a brief calculation of the numerical strength of the ranks—that is, the number of insects passing in a given minute, when multiplied by the period of time, two hours, during which the hastening hosts were in transit—produces the impressive though probably underestimated total of three hundred and sixty thousand." Mr. Shannon, who has given particular study to the migrations of insects, states that the principles and laws governing the better known bird migrations have a remarkable parallel in the annual movements of certain members of the insect world, both birds and insects being influenced by meteorological and geographical conditions which deflect and determine the routes, and the psychologies of both birds and insects reacting to the traveling impulses which are unsatisfied in some cases with anything less than a world-wide distribution.

How far south these roaming monarchs go, or whether they return north in the spring is not a matter of record. It is a known fact, however, that no specimens have been found in New England during the winter, although adult specimens are common in May. This would seem to indicate that they had returned from the south, and it is fairly generally accepted that such return migrations do take place. On the other hand, their good condition seems strong testimony that adults found north in May have not made any such long journey.

The object of the long migration also remains a mystery. According to Dr. F. E. Lutz, Associate Curator of the Department of Invertebrate Zoology of the American Museum of Natural History of New York City, the cause cannot be food shortage; for when the migration starts flowers are plentiful and food abundant. Moreover, adult butterflies take but little nourishment at any time. The article above referred to suggests that the insects travel south for further breeding, the swarming movement being supposedly connected with a psychological impulse to wander coexistent with the breeding instinct. This theory, however, does not account for the apparently definite and traditional routes followed. Due no doubt partly to its roaming habits and partly to its protection from birds, the monarch butterfly is gradually spreading over the entire earth. It is known in Australia, Java, Sumatra, the Philippines, Great Britain and the Cape Verde Islands.



A strange fish with Arabic inscriptions upon its tail

### The Treatment of Chemical Subjects by the Daily Press

A RECENT flagrant specimen of the unskillful treatment of a chemical topic by certain daily papers raises again the question of the methods generally adopted by newspapers in dealing with scientific and technical subjects. It is, indeed, only the latest of a long series of instances which might be quoted of the lamentable blunders that are committed when such matters are handled by technically unqualified writers and editors. Technical copy should before all things be accurate; but papers of a certain class have no use for a plain tale plainly told, and by them accuracy is invariably sacrificed to sensationalism. Little, if any, better is the "technical" information that is more or less directly connected with the advertising pages, whether from a company promoting point of view or as a simple trade puff; in either case, to employ the lively mixed metaphor which once came to our notice, we can "see the cloven hoof of the man with the axe to grind, peeping round the corner." Such matter is usually fulsome and one-sided; it is often written up from particulars supplied by interested persons, and its bias and lack of proportion render it absolutely unreliable. Of a slightly higher class are the technical articles by "expert contributors" which figure largely as makeweight to a page or so of trade advertisements in a familiar type of special supplement; but these should really all be marked "Adv."

With the exception of motoring and engineering topics, which are regularly entrusted to experts, the quality of the technical articles and paragraphs in the daily press is so poor that the discerning reader is impelled to remark: "If it cannot be done better than this, why not leave it alone altogether?" But can it not be done better? And can the organs of a vast commercial community afford to neglect such topics? It may be said that technical matters should be left to technical papers, where those that want them will know where to find them. But, for the sake of the advancement of the nation in technical efficiency, it is essential that a general interest should be maintained in the current development of scientific and industrial progress; and thereby let it not be forgotten, a great deal of extremely attractive copy may be provided. During the great banquet of the Eighth International Congress of Applied Chemistry in New York in 1912, the present writer heard Mayor Gaynor tender his apologies to the Congress for the small amount of notice which the New York papers had vouchsafed to its very important proceedings. Only a few minutes before, he had been pointing out to some American pressmen sitting near, what a number of capital "stories" they had missed in connection with, for instance, the fixation of atmospheric nitrogen, which might yet stand between mankind and starvation; the realization of vast fortunes by the utilization of waste products in, for instance, the cottonseed and soap industries; the fascinating combination of luck and skill exercised in the development of the synthesis of dyestuffs; and the militant rivalry of the producers of artificial india rubber. The frank American boys candidly acknowledged their chagrin at having overlooked the powerful human interest latent in these themes. The fact is undeniable that the public fully appreciates any trouble taken to increase its store of knowledge concerning objects and processes with which it daily comes in contact and that the intelligent mind is forever keen to know the answer to the questions "What is it made of?" and "How does it work?" Is it fair that those who thus seek the bread of sound knowledge should be put off with meretricious claptrap?

If, however, such questions are to be duly and truly answered there must be a considerable alteration in the methods of the press in dealing with them. The chief difficulty is that good technical writers are all too scarce. The expert contributor only too often comes under the profane condemnation assigned to the expert witness. He is either far too deeply immersed in his subject to make himself intelligible to the general reader; or he is a facile writer who can pour forth a flowing stream of words, wide enough in their scope, but most lamentably shallow and inaccurate. Of the two types the former is preferable, since solid fact is the essential basis whereon all permanent work must be founded. Only, between the weight of learning and the untrained understanding of the public, a qualified editor must know when and where to apply his blue pencil. This, however, is hardly within the scope of the limited omniscience of the ordinary news editor. What is needed is for all the press agencies to employ technical experts to keep them in touch with the latest developments in the principal branches of applied science; and for every paper that pretends to give information on such matters to maintain a list of acknowledged specialists to whom any articles or paragraphs in their particular branches may for a moderate fee be submitted; and such a list could readily be compiled from among the members of the Society of Chemical Industry. Only by these means can the public confidence be won and maintained; but it will be quite worth while if discreditable "stunts" may thereby be avoided and technical subjects can receive in our daily press the full and serious consideration which they indubitably merit.—*Jour. Soc. Chem. Ind.*

### Use of Stereoscope for Viewing Contour Maps

H. HUBERT in *Comptes Rendus*, proposes the use of a stereoscope for giving a clear idea of the contours of two or more superposed surfaces such, for example, as the ground surface and one or more geological strata. It is suggested that the maps be drawn by hand to a much-enlarged scale and then reduced photographically.

# Anomalies of the Animal World—Part XI

## Flying Frogs and Other Curious Batracians

By Dr. R. W. Shufeldt

It was Brongniart who, in 1799, pointed out the very marked differences existing between the frogs and such forms as the salamanders, termed by him *Batrachia*, and the true reptiles.[1] Five years thereafter Latreille separated Brongniart's *Batrachia* from the true reptiles (Class *Reptilia*) as a group of equal value, and for which he retained the name *Amphibia* of Linnæus.[2]

At the present time we relegate to the *Amphibia* three very distinct group of animals, namely: the newts (*Urodela*), the frogs and toads (*Anura*), and the *Cæciliæ* (Peromela), the last being peculiar creatures with snake-like bodies and totally devoid of limbs; they will not, for the lack of sufficient space, be touched upon in the present chapter.

As in the case of birds, we find among any of the Batrachia or amphibians, species that are considered to be "ordinary" types, as distinguished from those that may present something anomalous about them, whether such anomalies are referable to their structure, appearance, habits or otherwise. For example the common little newt of the ponds of the eastern parts of the United States, westward to the valley of the Mississippi River (*Dimyctylus viridescenes*), is an ordinary urodele, presenting, *as a newt*, nothing peculiar or anomalous. So, too, for the common bullfrog and the common American toad among the *Anura;* they are the most ordinary of amphibians.

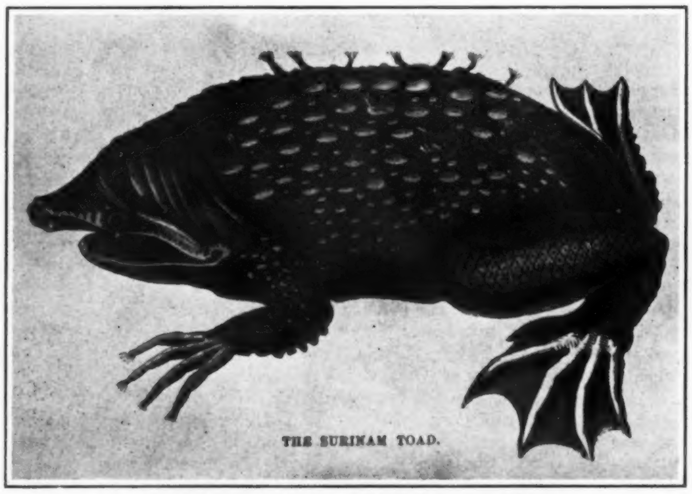
One of the best known, and probably the most notable of the family *Urodela*,[3] is the Congo "snake" or mud-eel of the southern lagoons, an animal studied by me both in nature and in captivity.[4]

On the rice plantations and elsewhere in the South, the Negroes kill this innocent Siren by the hundreds every year, claiming that it is the most venomous of reptiles. Personally, it has never been my good fortune to confirm a single one of their stories about any one dying from the bite of the animal, and obviously for a very good reason. At one time there was ten dollars offered by me to be paid to any one producing the full record of a well authenticated case of death from the bite of the "Congo snake." This reward never had to be paid; though one Negro, more mercenary than well informed in such matters, did bring forward a case of an hysterical, old colored woman who had been bitten several years previously by a Congo eel, dying *six months* after the infliction of the wound in "*spasms!*"

At one time, a medium-sized specimen of this Siren was kept by me in a large tub of water, and on several occasions his habits and actions were extremely anomalous. In handling him, he rarely offered to bite, unless the examination was prolonged or roughly conducted. Then he would curl up, slowly open his mouth, and make an awkward lunge at the fingers or hand that held him. Sometimes he would only open his mouth and hiss in a subdued manner. On one occasion, however, this interesting amphibian succeeded in getting out of his tub during the night; when found by me in the morning he was in a distant part of the room, and he snapped at my hand several times before he was secured.

It was amusing to observe the way in which he managed to leap out of his place of confinement—a large tin bath tub with the water seven or eight inches below the brim. He swam round and around with increasing rapidity till the necessary impetus was acquired, when he would prettily make a sort of spring over the side of his tub onto the floor, where he would squirm around like an eel until he was replaced. In such situations, he uses his legs to the full extent of which they seem capable. In the water, too, these members are constantly being brought into use, the fore-pair when he desires to move very slowly forward, in which case he may or may not—generally not—use the hind pair in aiding the action. The fore-pair are also used alternately to push himself one way or another when he wishes to change his course. A common use for the hind pair is to throw them forward and brace them against the ground he may be passing over, in order to check his onward movement, either partially or entirely. In swimming about, he has all the appearance of the common eel, and during these times he draws both pairs of limbs close to his body, when his action is graceful and interesting to behold.

Flying frog of Borneo, after Wallace




The Surinam toad, reproduced from an old cut

When these sirens are at rest, they either stretch out, in gentle curves, sluggishly along the bottom, or what is not very uncommon for them to do, curl up tightly, the latter two-thirds of their lengths in a spiral manner, while the head and remaining third is protruded forward in a direct line. In this curious position they float near the surface, the head being lowermost. If two occupy the same vessel, they often curl about each other in a rather affectionate manner; but I have never witnessed them quarrel or fight. Once it occurred to me to throw a dead king-snake into the tub of my first small specimen, the snake being at least three times as long as the siren. Imagine my surprise to see him fly at the intruder, seize him just below the head, straighten out as stiff as he could, then rapidly whirl around, as a drill does, causing the dead snake to be spirally coiled about his body. A moment of quietude followed this strange manœuvre, during which one could see a crunching movement going on, on the part of the siren. But, finding that his enemy showed no resistance, he slowly let go his hold, and, freeing himself from the dead snake's coils, swam about the tub without paying him any further attention. In a few moments, however, upon repeating the experiment, he made the same attack, and with just as much vigor as before. But all subsequent trials failed, and he could not be induced to take further heed of such a harmless enemy.

This siren will eat crayfish in confinement, but cannot be persuaded to take anything else, although raw meat is the common bait used by the negroes in catching them for me for specimens. Sometimes before a meal—or maybe after—your captive will swim gracefully about his limited quarters, and occasionally rising to the surface will stick his nose out of water and give vent to a loud blowing sound that may be heard anywhere in a large room, even if conversation be going on.

The sirens or "Congo eels" belongs to the family *Amphiumidæ*, while the Necturus or mud-puppy, so common in the Great Lakes, belongs to the family *Procteidæ*, and the "hell-bender" of the Ohio valley and southward to the *Cryptobranchidæ*.

Then the world's fauna contains about twenty-five species of amblystomas (family *Amblystomidæ*), in this group belongs the Axolotl of Mexico and the southern parts of the United States, which Cuvier thought to be a large "Eft-tadpole." Hundreds of these curious animals, known as amblystomas, were studied by me in New Mexico, a great many years ago, when their transformations were observed under all conditions.

The fact that Cuvier and others of his time believed them to be Eft-tadpoles was due to their having been noticed to reproduce their young during the *tadpole stage*. This is what the late St. George Mivart, a distinguished British naturalist, was pleased to say about these axolotls in his excellent little work "The Common Frog": "For some years, individuals of this species have been preserved in the Jardin des Plantes at Paris, and a few years ago one individual amongst others there kept was observed, to the astonishment of its guardian, to have transformed itself into a creature of quite another genus—the genus *Amblystoma*, one rich in American species. Since then several other individuals have transformed themselves, but without affording any clue as to the conditions which determine this change—a change remarkable indeed, resulting as it does not merely in the loss of gills, and the closing up of the gill-openings, but in great changes with respect to the skull, the dentition, and other important structures.

"There is, moreover, another very singular fact connected with this transformation. It is that no one of the individuals transformed (although we must suppose that by such transformation it attained its highest development and perfection), has ever yet reproduced its kind, and this in spite of every effort made to promote reproduction by experiments as to diet, and as to putting together males and females both transformed, also transformed males with females untransformed, and males untransformed with females transformed. Indeed, the sexual organs seem even to become atrophied in these transformed individuals. Moreoever, all this time the untransformed individuals have gone on bringing forth young with the utmost fecundity, no care trouble on the parts of their guardians being required to effect it."

With respect to the transformations of the axolotl, much has been learned since Mivart published his "Common Frog" (1881), and many important facts have been added to its life history.

[1] Brongniart, Alexandre. "Essai d'une classification naturelle des reptiles." This brochure was not published in full until 1803, and two years after it appeared in the "Memoires presentés à l'Institut par divers Savans."

[2] Latreville, Pierre André. "Familles naturelles du Régne Animal (1825). Latreille was a distinguished French zoölogist who died in Paris in 1833.

[3] From two Greek words meaning "tail" and "conspicuous"; that is, amphibians with tails both large and long.

[4] Shufeldt, R. W. "The Habits of *Murænopsis tridactyles* in captivity, with observations on its habits." *Science*, Vol. 2, No. 27, Camb., Mass., Aug. 10, 1883, pp. 159-163. Illustrated.

At the meeting of the Zoölogical Society of London, which took place on the 20th of May, 1913, Mr. E. G. Boulenger, F. Z. S., read a paper under the title of "Notes on the Metamorphosis of the Mexican Axolotl (*Amblystoma tigrinum*)," an abstract of which is published in No. 122 of the Abstracts of the Society. The material upon which Mlle. de Chauvin, a student of these animals, made many of her experiments in Germany—to which Mr. Boulenger alludes—was collected by me, and experiments of my own, which were conducted in New Mexico, where these forms were present in nature in immense numbers, subsequently appeared in *Science* (Vol. VI, No. 138, Camb. Sept. 25th, 1885, pp. 263, 264, and *Ibid* Vol. VIII, No. 194, New York, Oct. 22, 1886, p. 367).

Mr. Boulenger, through his study of this species of *Amblystoma*, is convinced that Mlle. de Chauvin was correct in the deductions she drew from the experiments she had made, and that those of Dr. Powers were erroneous, the latter having also made a series of observations on axolotls. My own experiments were made in my laboratory and on the animals in nature, the one series being a check upon the other.

What Mlle. de Chauvin published in *Science* (No. 130) at the time was read by me, and it was admitted in my article that my own observations confirmed those of that talented observer. My results were arrayed under nine conclusions, and in some instances, these conclusions seem to be more or less at variance with those arrived at by Mr. Boulenger. He remarks that "Axolotls will, with few exceptions, transform if placed under conditions which force it to breathe air more frequently than usual"; whereas in my first conclusion it is stated that "Axolotls are more readily converted into Amblystomas if kept in water containing but little air, and *vice versa*."

Again, Mr. Boulenger found in his experiments that "Starvation, irregular feeding, and temperature have no influence on the metamorphosis." In this connection it is stated in my article that "Axolotls are very voracious creatures, and eminently carnivorous. They are very fond of raw meat, and, upon the slightest provocation, they will feed upon each other." That "the metamorphosis is hastened by regularly supplying the animals with plenty of proper food. And, what is still more interesting, when they are thus treated, it markedly affects the appearance of the transformed Amblystoma."

Mr. Boulenger found that the temperature had no influence upon the transformation with respect to time, and my researches do not confirm this result.

As a matter of fact, as rapidly as my experiments were made upon these forms in my laboratory, they were compared with what was to be observed with respect to the species in its natural environment in the country where they occurred in countless thousands, as will be noted from my contribution to *Science*, which is altogether too long to be reproduced, even in part, in the present chapter. My experiments enabled me to thus study them—in fact, two species of them—in nature, under conditions that repeated those artificially produced in the case of a large number of them in my laboratory. Not only for a brief period, but for two or three years, at all seasons, in shallow and in deep water, and at very different temperatures. At great variations in the matter of kinds and amounts of food; during very sudden draughts, when they suddenly found themselves in immense numbers on dry land, with nothing to feed upon save themselves, and under various other conditions.

One of the most anomalous forms of this family is the "typhomolge," a *blind*, nocturnal-like salamander that is found in certain caves in Texas, the habits of which are, as yet, but very imperfectly known.

There are still other very remarkable urodels found in other parts of the world, as the cave "olm,"—a proteidid inhabiting certain caverns in western Austria, and the Giant salamander of Japan, an enormous cryptobranchid, often attaining a length of three meters or more.

Passing to the Frogs (*Anura*), and having the fact in mind that in every other class of the vertebrata we met with more or fewer examples that enjoyed aerial locomotion—the birds as a matter of course—but then, too, fish, reptiles and mammals; so we might be pardoned were we to look about us for a volant frog or one fitted to fly through the air.

Such a creature has now long been known, and known as "the Flying-frog of Borneo," a species discovered by the venerable naturalist Alfred Russel Wallace. In his fascinating work, the "Malay Archipelago" he says: "One of the most curious and interesting creatures which I met with in Borneo was a large tree-frog (*Rhacophorus*) which was brought me by one of the Chinese workmen. He assured me that he had seen it come down in a slanting direction, from a high tree as if it flew. On examining it I found the toes very long and fully webbed to their extremity, so that, when expanded, they offered a surface much larger than the body. The forelegs were also bordered by a membrane, and the body was capable of considerable inflation. The back and limbs were of a very deep shining green color, the under surface and the inner toes yellow, while the webs were black rayed with yellow. The body was about four inches long, while the webs of each hind foot, when fully expanded, covered a surface of four square inches, and the webs of all the feet together about twelve square inches. As the extremities of the toes have dilated discs for adhesion, showing the creature to be a true Tree-frog, it is difficult to imagine that this immense membrane of the toes can be for the purpose of swimming only, and the account of the Chinaman that it flew down from the tree becomes more credible."

Other frogs of this genus, smaller in size, and with less developed webs between their toes, are also known to science.

There is another anomalous frog found in France (*Pelobates fuscus*), both sexes of which croak in a peculiar way when one pinches their thighs. The noise sounds like the mewing of a cat, and if the pinching is persisted in, the frog emits a most outrageous odor something like garlic, which is so strong that it brings tears into the eyes of those within the range of its influence.

There appears to be but one group of "toads," the bite of any one of the species of which is said to be venomous; these are the giant horned frogs or toads of South America. As a rule it is a character confined to various ophidians; very doubtfully to a lizard (*Heloderma*) and in the case of some mammals, man included, whose saliva seems to gain venomous properties during fits of violent anger. The spur of the Duckbill has already been alluded to in this connection in a previous chapter.

Mivart says there is "A small Frog, by no means uncommon in France and Germany (*Alytes obstetricians*), which has a very singular habit. The female lays its eggs (about sixty in number), in a long chain, the ova adhering successively to one another by their tenacious investment. The male twines this long chaplet round his thighs, so that he acquires the appearance of a courtier of the time of James I. arrayed in trunk hose and puffed breeches. Thus encumbered, he retires into some burrow (at least during the day), till the period when the young are ripe for quitting the egg. Then he seeks water, into which he has not plunged many minutes when the young burst forth and swim away, and he, having disencumbered himself of the remains of the ova, resumes his normal appearance."

This is not quite as anomalous, however, as is the case of the American tree-frog, known to science as *Nototrena marsupiatum*. Here we have a species in which the female has a pouch over the entire dorsum with an opening behind. This constitutes a pocket for her eggs during the period of their development.

In this particular, perhaps the most remarkable batrachian known is the Great Toad of Surinam in tropical America (*Pipa americana*). This is a species wherein the skin of the entire back of the female becomes conspicuously thickened as the laying season comes on. This skin is quite soft and notably loose. As soon as she lays her eggs, the male performs the duty of picking them up and depositing them on this soft skin of the back of the female, which soon closes over them, all but a small area on each egg. Here the latter develop so far that, when the young emerge from their "cells," they are perfect miniatures of the mother. Here, although they pass through the entire tadpole stage, they are doomed to not swim about in the water during it, as the tadpoles of most all other frog do.

An illustration of this toad is herewith presented, but others, and better ones than this, may be seen in various works on zoölogy, as for example the one given by Mivart in his booklet on the "Common Frog" (Fig. 11), or in Figs. 246 and 247 of Davenport's "Introduction to Zoölogy," Fig. 247 giving a section of the skin through three cells containing the developing frogs.

Toads are said to live to be several centuries old; but the myth of their doing so—encased in solid rock—has long ago been exploded. Saville-Kent mentions "A very beautiful Australian species, abundant in Tasmania and Victoria, and appropriately named the 'Golden Tree-frog,' has its grass-green overcoat thickly overlaid and embroidered with, as it were, the purest beaten gold."

In a previous paragraph of this chapter, the Horned Frogs or Horned Toads (not *Phynosoma*) of South America were mentioned. There are fully a dozen species of these known to science, and they bear the name they do from the fact that the upper eyelids support a horn-like process. The biggest one is found in Brazil, it being fully eight inches in length. They are truly gorgeous-appearing creatures, being colored bronzy-green and yellow, and blotched with big, chocolate-colored spots, interspersed between which are lines of brilliant claret-red.

Mr. W. H. Hudson, in his most entertaining book, "The Naturalist in La Plata," gives us a fine account of a species of these frogs that occurs on the pampas of Rio Colorado in Patagonia and northward. He says: "In the breeding season it congregates in pools, and displays extraordinary vocal powers, which are exercised at night. The notes uttered are long, resembling those of a wind instrument, and are so powerful that on still evenings they may be heard distinctly a mile off. After the pairing season the frogs disperse, and, retiring to moist places, bury themselves just deep enough to leave their broad green backs on a level with the surface. The eyes, under these conditions, look out as from a couple of watch towers, and are on the *qui vive* for any approaching prey. This consists of any moving creature which they can capture, such as other frogs and toads, birds and small mammals. In very wet seasons, they will frequent the neighborhood of houses, and lie in wait for chickens and ducklings, often capturing and attempting to swallow objects much too large for them. When teased, the creature swells itself out to such an extent that one expects to see it burst. It follows its tormentors about with slow, awkward leaps, its vast mouth wide open, and uttering an incessant harsh croaking sound. When they bite, these frogs hold on with the tenacity of a bull-dog, poisoning the blood of the creature seized with their glandular secretion."

Mr. Hudson tells us of two cases in which to his personal knowledge horses succumbed to the bite of this frog. One horse was lying down and was seized on a loose fold of skin on its belly; the other horse was grabbed on the nose by the frog while the former was grazing. In each case the frog was discovered *dead*, with firmly closed jaws, holding on to its victim—both of the horses having succumbed to the bite of this venomous amphibian. "It would seem," as Mr. Hudson points out, "that they are sometimes incapable of letting go at will, and, like honey-bees, destroy themselves in these savage attacks."

There is also a blue or bicolored frog in South America, with white underparts. Some of its habits are quite at variance with those of ordinary frogs. Mr. Saville-Kent says that there is a green frog in Queensland that makes a home in your house and destroys many noxious insects, and that it becomes so attached to the place that it cannot be driven away. You may turn him out over and over again, even carry him off on horseback, full half a day's journey, into a trackless wilderness; but back he will be in your house the next day or the day after.

## Evaporation from Surfaces of Crystals

Assuming that the atoms are regularly grouped in a crystal and held together by quasi-elastic forces, which are of different magnitude in different directions, we might expect evaporation from a crystal to take place in preferential directions. This view is not supported by experiments. The author mounts a crystal (sulphur, zinc, silver, stibnite), in such a way between a block of iron and a thin foil of Pt that a smooth crystal or cleavage face of 2 or 3 mm. diameter is exposed, suspends it by Pt-wires inside a sphere of glass, 6 cm. in diameter, heats the crystal by the current through the wires and cools the sphere in liquid air. The suspension is such that the crystal lies in the spherical surface. The vapor condenses on the glass in a thin film apparently of uniform thickness, while a pit (0.15 mm. deep in the case of sulphur), forms in the exposed face which turns grainy. Not all the atoms in the surface seem to assume the vapor form at the same time, therefore, and the evaporation does not appear to be limited to preferential directions. The cosine law, which the author had shown to hold for incident atoms of Hg, Zn, Cd see Abs. 371 (1909), 1317 (1911), 1357 (1916), must thus hold also generally for atoms evaporating from a crystal, as otherwise there could not be equilibrium.—Note in Sci. Absts. on an article in *Ann. d. Physik*, by M. Knudson.

## Quantitative Micro-analysis

Instances are given of the applicability of micro-analysis, *i. e.*, the determination of very small quantities of constituents when using minimum amounts of material. Provided that accurate measuring vessels and a sensitive balance are employed, it is quite possible to determine accurately the extractives and ash in 5 c.c. of wine, etc., small capsules formed out of platinum foil are used for the purpose. Micro-analysis is particularly suitable for the determination of phosphorus, since this yields a precipitate of ammonium phosphomolybdate having a weight 55 times that of its phosphorus content. In the determination of small quantities of carbon, it is preferable to use a gas-volumetric method, the volume of carbon dioxide produced from the carbon being readily determined; the same applies to nitrogen, ammonia, and hydrogen. Very small quantities of metal in oxides (*e. g.*, zinc in zinc oxide) may be determined by measuring the volume of hydrogen evolved when the substance is treated with an acid.—Note in *Jour. Soc. Chem. Ind.*, on an article by C. Reichard in *Pharm. Zentralh.*

### Radiation from System of Electrons*

In concluding the spring term of the Royal Institution on Friday, March 22d, by a discourse on "Radiation from System of Electrons," Sir J. J. Thomson, P.R.S., O.M., dwelt on features to which he had not made any particular reference in the course of his lectures on "Problems in Atomic Structures," which he completed on the following day. In a certain way this discourse hence supplements the lectures, and the introduction to our brief notice of the discourse will perhaps best be understood if we refer our readers to our more detailed account of the lectures (S. A. Supp. No. 2211, p. 306, May 18). We also for convenience of reference reproduce here the Figs. 1 and 2 published with our article just referred to. If light coming from O struck two electrons, A and B (Fig. 1), Sir Joseph said, it would be scattered in various directions, and the energy received at some points P would depend upon the relative lengths of the two paths, O A P and O B P. To demonstrate the scattering Sir Joseph let the beams of the lantern fall on two knitting needles, whose lengths corresponded to A B, held obliquely in the beam; on the screen two intersecting luminous arcs of the two needles were produced. With perfect coincidence of wave-length and phase the radiations from A and B would reinforce one another. The curve indicating the intensity of the scattering in various directions was a damped curve showing maxima and minima (as in Fig. 2); the maxima and minima would lie on cones and the apex angles, and thus the diameters of the rings which they produced on a screen, depended upon both A B and the wave-length, $\lambda$ or rather upon the ratio $d/\lambda$ (if A B $= d$); an alteration of either $d$ or $\lambda$ would alter the cone angles. That was shown in the demonstration (mentioned on page 306 *ante*) by holding a glass plate powdered with lycopodium in the lantern beam; the light spot was surrounded by colored rings. The particles had to be of very uniform size for this purpose, Professor Thomson remarked; when they were not uniform, the effect became indistinct, and of various powders he had tried, silica among them, he had found plant spores most suitable. A mist of water vapor or alcohol vapor, produced in a spherical bulb by expanding the vapor, showed these diffraction rings also; they were occasionally noticed on the Brocken spectre—when an observer saw his own shadow on the clouds.

Each pair of electrons, Sir Joseph continued, then gave curve like Fig. 2, or rings and, if ring systems intersected, spots. When we dealt with atoms we had many electrons and we could only observe and calculate average effects. By analysis of the photographic records the effects of different pairs could be separated, however, and the different lengths $d$ could be estimated. When four electrons were grouped at the corners of a square, the side of the square would be smaller than the diagonal, which also represented an A B in another direction; when the four electrons were at the corners of a tetrahedron, all the A B would be the same; thus the examination gave some clue as to the grouping of the electrons. As regards the number of electrons concerned, the suitable conditions for reliable calculations were more fully explained on the previous occasion (page 306 *ante*) to which we have already directed attention. The condition, that A B should be large by reference to $\lambda$, was not always heeded apparently in X-ray studies, the

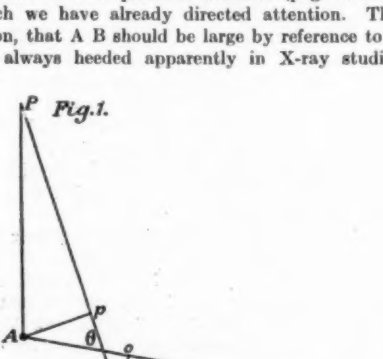


*Fig. 1.*

lecturer thought, and the absolute values obtained seemed doubtful for this reason. If all the electrons pointed the same way, the effect would be most brilliant. That, and the change of iridescence colors with the angle of incidence, could be demonstrated with ordinary light; Sir Joseph let the light of the lantern beam fall on some birds—a quezal, colibris, etc.—which were seen to shine in iridescent colors, brilliant green and blue, or dull greyish blue, according to the incidence. Specimens of mother of pearl and crystals of potassium chlorate, exemplifying the same effects, were on the table. When there were many electrons, it was not always possible, of course, to bring them into proper coincidence for reinforcement; but with proper $d$ a series of spots would be obtained. The $d$ here represented the side of the elementary cube of the space lattice of a crystal; Sir Joseph did not enter further into these points. To understand the problem, he said, one might think of a grating in three dimensions. Their effect could not be indicated by a drawing, but he could show what resulted when a ray of light fell upon two gratings so superposed in the same plane that the rulings of the one grating were at right angles to those of the other. Eight colored light spots were then seen about the bright center marking the incident ray; the spots were occupying the four corners of a square and four points half-way between the corners, and each spot was oval, the blue being turned inward, the red outward.

The difficult analysis of the X-ray patterns of spots obtained in crystal study, Sir Joseph proceeded, was generally performed by elaborate geometrical methods; but he had found a much more simple arithmetical

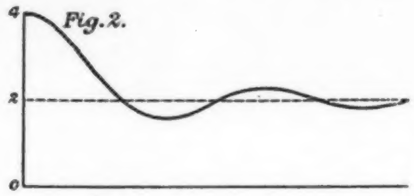


*Fig. 2.*

method. That method resolved itself into finding three numbers, such that their squares summed up to the square of the given number. The number started from was the ratio of the side of the elementary cube, which represented the distance A B between the electrons, to the wave length, or $d/\lambda$. Suppose $d/\lambda = 5$, then $5^2 = 25$ might be expressed $5^2+0^2+0^2=25$, or $4^2+3^2=25$. Now each of the rations $\frac{5}{5}, \frac{4}{5}, \frac{3}{5}, \frac{0}{5}$ represented a cosine direction of a spot, which was defined by the three cosines of one resolution on an orthogonal system of coordinates. Similarly $9^2=81$ might be resolved into $9^2+0^2+0^2=8^2+4^2+1^2=7^2+4^2+4^2=6^2+6^2+3^2$; in this case the cos were, *e.g.*, $\frac{8}{9}, \frac{4}{9}, \frac{1}{9}$. The order of the terms $\left(\frac{4}{9}, \frac{8}{9}, \frac{1}{9}\ e.g.\right)$ being immaterial, each of these resolutions gave six spots, unless two of the fractions were equal or equal to 0, when there would be fewer spots. The spot grouping was kaleidoscopic. If a spherical quadrant were drawn, then any point on the quadrant would produce six images with respect to the planes bisecting the solid right angles—unless the point were lying on one of the boundary planes, when one of its coordinates would be 0; the whole sphere about a point would comprise eight of these quadrants, and there would thus be $8\times6=48$ spots (or fewer). Professor Thomson exhibited some of the original photographs of Laue, whose studies of the diffraction of Röntgen rays orginated this study of crystal structure by X-rays, and also some which A. W. Hull had quite recently obtained.* The method of Hull, we may mention, resembles that which Debye and Scherrer have used since 1915 in studying the structure of many crystalline substances and also of amorphous carbon, its advantages being that no crystals are required, but that particles of the finely powdered substance suffice which will be, and must be, in random arrangement. Debye forms small compressed cylinder of the substance; Hull places the powders in a small glass tube which he rotates about the longitudinal axis to which the beam of monochromatic X-rays is normal.

Concluding, Sir Joseph pointed out that the trouble was that the center of the disturbance was supposed to be the atom, while the electrons were the centers, and though the arrangement of the electrons could be studied by using X-rays of different wave lengths, the difficulty remained that we had really to deal, so to say, with two sets of gratings: firstly, the grating due to the arrangement of the atoms in successive parallel planes; and secondly, the grating formed by the electrons in the atom. Both these sets gave systems of spots. In some direction the two would coincide, and the electrons and atoms would conspire in the effect. That would not necessarily be a spectrum of the first order, however; it might be some spectrum of higher order, complicating the effects. He regretted that he had spoken of things that might be done rather than of things done; but there was little time for research just now.

*From *Engineering*.

*Physical Review, Dec. 1917, pp. 661 to 696.

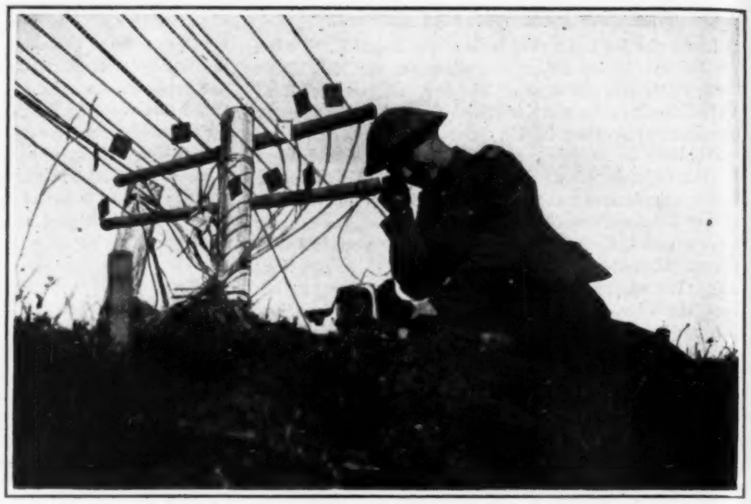
### Danger in Flying Through Clouds

It may not generally be known that there have been such a large number of fatal accidents during the last three years entirely due to flying through clouds, and I consider this subject wants going into pretty carefully. The accidents to which I refer have not been questions of a want of height; the machines have become hopelessly out of control. I will give you an instance which happened to myself a few weeks ago in the west of England. You will then realize why I consider this is a serious matter requiring particular attention. I set out on a very cloudy, windy day to do a test climb to 10,000 feet on a late type two seater. I had so often on previous occasions succeeded quite comfortably in reaching this height in spite of cloudy, overcast days by pushing up through the clouds, usually only a matter of a few minutes, into bright sunlight and the bluest of skies, and, after reaching the desired height, coming down again through the clouds, having flown by compass and time. On this particular day, however, the wind was very gusty, and on reaching 1,200 feet we got into dense rain clouds, but carried on to beyond 5,000 feet, still in the cloud, when the compass apparently began to swing (really it's the machine that begins swinging, not the compass), and efforts to check the compass had the effect of causing it to swing more violently in the other direction. The air speed then rushed up far beyond normal flying speed; all efforts to pull her up checked her only slightly; then the rudder was tried, back went the air speed to zero; there was an unusual uncanny feeling of being detached from the machine, and I knew her to be literally tumbling about in the clouds. All efforts to settle down again to a straight flight seemed to be unavailing, until we emerged from the cloud very nearly upside down. Assuming control again was then an easy matter. This sort of thing has happened to me more than once, and, in the Flying Corps vernacular, "it puts the wind up you," and it has happened many times with other pilots. In some cases they emerge from the clouds in a spin, others are known in which the planes have collapsed under the strain of the sudden pull-up from the vertical nose-dive. A few days ago, a squadron-commander told me that on one occasion when in France everything fell loose in his machine fell out while in a cloud. A week or so ago, on the South Coast, a machine disintegrated in a cloud and the main planes landed half a mile from the fuselage. From my own experience, this is a very unpleasant state of affairs, and I avoid clouds when possible. First of all you must realize that in a cloud you see nothing whatever but your machine around you. There is no fixed point visible. The only means by which you can tell if you are flying in a straight course is by your compass and your air speed. The compass should give you your direction horizontally your air speed your direction vertically. The first thing that happens, and very readily too, if windy and bumpy is that your compass card will begin to move slightly. It really appears to you that the compass was suddenly affected by the cloud, and you are still flying straight ahead. How often you hear a pilot say that as soon as he got into a cloud his compass started spinning. The moment the compass starts moving it requires extremely delicate ruddering to get it back to a steady position; in fact, one invariably over-corrects the compass movement and so the trouble begins. Once the compass starts on a good swing I have found it nearly an impossibility to get it steady again until out of the cloud. Before your compass starts to move your machine has already started to turn. You rudder the opposite way to check it, over-correct it, and turn sharper the other way on to a bank turn; then the nose drops and speed goes up. Pulling back your elevator lever has little or no effect, for if you are banked above an angle of 45 degrees the elevator becomes the rudder. All this occurs without the pilot being in the least bit aware of the position his machine is taking relative to the ground. The instruments available are of little service once he loses his control.

Of what use is his air speed indicator to him indicating 150-m.p.h. if the machine is on a spinning spiral and he imagines that he is merely descending too fast on a steep, straight glide? He naturally tries to pull up, but with no effect. The bubble does not help him, as centrifugal force will send that anywhere. It may be argued that if a stable machine is left alone under these circumstances it will right itself eventually and assume a normal glide. It very likely would if the pilot could steel himself to let it entirely alone, but before it did so it would have to be left to do a sheer vertical nose-dive for some moments, and in these days of big weights and little head resistance one is liable to attempt to pull out too suddenly from the dangerous high rate of speed attained on this dive. What I want to see fitted is an instrument which will show a constant vertical or horizontal line and be independent of centrifugal force. I have no ideas upon the subject nor suggestions as to how this is to be brought about, unless something in the nature of a small gyroscope driven by an air-screw could be employed in some way to meet the requirements of flying in clouds, but until something is provided so the pilot can see a fixed line, I think we shall continue to have accidents from this cause.—Abstract from a paper before the Aeronautical Society of Great Britain by Capt. B. C. Hucks.

The Gilliams Service

An Italian telephone station in the high Alps



A typical British field telephone line. Testing circuits

# Battle Telephones

## How Orders and Information Are Transmitted at the Front

OUTSIDE the service, almost unknown, are the daring deeds of the gallant linemen of Uncle Sam's Signal Corps who fix battle telephone and telegraph wires making cooperation of infantry and artillery and perfect coordination of all branches of the service possible. To keep the means of communication open on the modern battlefield, these men undergo the greatest risks and are frequently killed while at work, but they persevere, restoring wires as fast as the enemy shells disrupt them.

In the battle of Seicheprey, it is said the Germans concentrated their artillery fire on the American telephone and telegraph wires which were in consequence cut many times.

But our boys were not to be beaten in that way and almost as fast as the enemy shells disrupted them the men of the Signal Corps, in the face of the heavy bombardment, restored the wires.

This is the latest example of the dangerous work of the daring linemen who fix battle telephones. It is heroic work, yet outside of the service it is almost unknown, although one of the most important of all scientific ends, of modern warfare.

When the fighting begins over the top, with the charging infantrymen go the linemen, generally with the second wave, in charge of an observing officer and immediately in magnificent disregard of terrible danger they begin coolly stringing lines behind the advancing first line, so that the artillery may be kept informed of the advance or told to concentrate their fire on a certain Boche gun crew who, in a well protected position, are making things obnoxious for our boys.

Usually the linemen as quickly as possible make for a point where they can establish an observation post and as they pass on and through the enemy's barrage they unroll their line and one of them carries a field telephone through which they somehow manage in the din of battle, to make themselves heard.

That telephone is like a battle flag, and many a man goes down with it, only to have it picked up and carried forward by another of these non-combatant troops whose business is only to serve, not to fight.

When the battle moves forward rapidly and the telephone and telegraph wires have to be moved at top speed to keep up with the advance the importance of the signal service is demonstrated in a way as impressive as the onward rush of light artillery going into action.

A division is ordered to move to another position; with it go two cable wagons, carrying cable which is attached at one end to a permanent line. They are autocrats in their way, these wire men, and no one is permitted to interfere with the swift execution of their work; they have the right of way over troops and supplies, and at a rapid trot dash through the roads, the men on the wagons paying out the cable. Back of the wagons ride men on horseback who with hooked sticks toss the cable into ditches and behind hedges out of the way of troops and transport wagons, while further back other horsemen tie the line and make it secure.

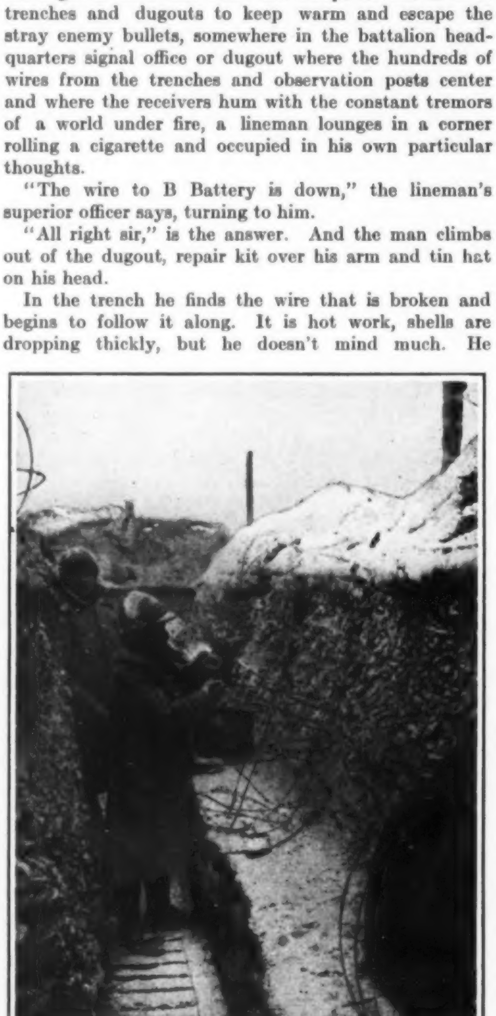
They are in the forefront of every advance and in the retreat are sometimes the last to leave the front line, where they stick to the end of their wires under terrific shell fire until ordered to rejoin their commands if they can get through alive.

At another time when no real battle is raging yet No Man's Land is swept by constant artillery fire, when the night is dark and when the troops huddle in their trenches and dugouts to keep warm and escape the stray enemy bullets, somewhere in the battalion headquarters signal office or dugout where the hundreds of wires from the trenches and observation posts center and where the receivers hum with the constant tremors of a world under fire, a lineman lounges in a corner rolling a cigarette and occupied in his own particular thoughts.

"The wire to B Battery is down," the lineman's superior officer says, turning to him.

"All right sir," is the answer. And the man climbs out of the dugout, repair kit over his arm and tin hat on his head.

In the trench he finds the wire that is broken and begins to follow it along. It is hot work, shells are dropping thickly, but he doesn't mind much. He follows the wire down a communication trench and then for a long time out into the open, where he has to crawl along looking for the hole that will mark the place where the line has been broken.



The Gilliams Service

Wiremen stringing telephone lines in a French trench

He gets nearly there when a stray shell ends his career. After a time, back in the dugout the first man failing to report another one is sent out, perhaps he is luckier than the first repairman and finds the break.

Then he has to sit down in the shell crater, the smash of bursting shells so close that sometimes he is half buried in dirt, calmly making the connection that will enable the observation officer up front to get in touch with his battery again. If he gets back to the dugout he will be sent out again and yet again if the bombardment is heavy and wires frequently broken. Often for days and nights at a time these men are under fire, snatching a nap now and then in the dugout between breaks.

The linemen also have regular patrols, stretches of line which have to be constantly examined not only for breaks but also to make sure that they have not been tapped by enemy spies in such a way that every bit of information sent over them finds its way to the Germans.

One day not long ago, a lineman passing along a road in the Aisne district noticed a lot of cable lying at one side. He started to coil it up and found that a piece of wire had been tied to the main line. When he traced it he discovered that it ran to a haystack. He went on, tapped the line and sent in word to headquarters and an armed escort found a spy hidden in the hay with several days supply of food.

What the nervous system is to the human body the telephone and telegraph system are to the modern army which can not see, feel or move without them. It must be remembered that battle fronts today are conducted on a different principle from what they were in previous wars. Then the forces of opposing armies as a rule only extended a few miles, today a 100-mile front is common and the army commander wishing to move a portion of his line 50 miles away or to change the rapidity of his artillery fire or to receive information of enemy movements would be helpless without the aid science has given him, the electrically controlled slender threads of copper.

Despite the constant efforts of the Huns to prevent it somehow, all along the allied fronts, the lines are kept open all the time or are broken only for short intervals.

The tremendous use of the telephone and telegraph in the present world's war is partly the result of the impetus arising from the American application of electrical communication on a large scale in the Spanish War. Uncle Sam's Signal Corps as it now exists is a comparatively recent evolution.

The idea first arose in the mind of a young army surgeon, Albert James Myer. The office of signal officer of the army was created in June, 1860, the first of its kind, and Myer was appointed. He was sent with an expedition against Navajo Indians in New Mexico, and his crude apparatus at once demonstrated its worth.

When the Civil War broke out he was ordered East and opened a school for signallers, and in that was the definite beginning of the present Signal Corps. Wires were carried on horse or mule back then, the instruments were imperfect and telegraphic communication was a rare and precious thing. The service took on tremendous importance in the Spanish War and followed the troops through Cuba and the Philippines, and in China was the only means of communication for a week between Pekin and the rest of the world.

But the tasks that confronted our signal men in these wars were play compared to the work that is being done every day by these boys on the western front. They have an area to cover as big as some of our largest western states and they have gone at it with vigor and efficiency.

The hardy linemen who have strung lines and repaired breaks on the western plains or battled with great floods and storms in the Rocky Mountains have taken to this new work with a zest which is inspiring. On the founda-

**An American dugout telephone station in France**

**British engineers laying a main cable along a trench**

tion of the French system they are building a signal system that will be a model of its kind.

Up to within four miles of the front, construction is not different from what it is in the United States. The wires are strung on poles and most of the poles have been planted by the French. But inside the shell torn section that stretches at least four miles from the front, wires have to be protected by being buried from six to eight feet deep, so that only a direct hit by a large shell will disturb them. Within half a mile of the front not even this protection is sufficient, as the shells churn and rechurn the ground. Therefore all wires in this zone are duplicated and are strung along both sides of the trenches. Sometimes a trench wall is covered with wires.

These hundreds of lines back of the front are joined into main lines or cables which hold from twenty to fifty circuits. According to the present increase of our forces in France it will not be long before we will need probably 30,000 miles of circuit and at least 12,500 men will be necessary to keep it open and in repair.

Since we entered the war only a little over a year ago, it is surprising how quickly our linemen have advanced to the head as being the best wiremen on the battle front.

When our Signal Corps began to move to France, calls were at once sent out for more skilled construction men, linemen, engineers and other experts in all that goes to make up a successful telephone system at the front. Fortunately for Uncle Sam, hardy construction and repairmen of the various telephone and telegraph companies, men who have worked on the transcontinental line, men fitted to cope with the most difficult weather conditions volunteered in large numbers and among them were some of the foremost telephone engineers in the world, which perhaps explains why America leads in this important branch of war work.

## Early Potatoes

The vital necessity arises again this year of producing at home foodstuffs of all kinds on as extensive a scale as possible, and those who have ground or other means suitable for raising crops will be alive to the advantage of cultivating every available yard, not only to meet individual wants, but to provide against any national emergency. Others having glasshouses, garden frames, or whose garden nestles inside walls, may have crops of potatoes as early as the means at command will allow.

The potato is capable of being forced in a variety of ways, either in pots or boxes, or on made beds under glass; but the first essential is to procure tubers of the right varieties, and these are found among the short-topped early kidneys.

The next important point is the soil. I have found a good compost in an equal mixture of rotted leaves and sandy loam. When pot culture is intended a size that measures 9 inches to 12 inches across is suitable, and after a piece of crock has been put over the hole at the bottom it may be half filled with the compost; make it fairly firm, and on it plant three tubers in a triangle, with the eye end uppermost more than 1 inch from the side. Then fill up the pot to within a few inches of the top, the mixture for this purpose containing less leaves, and more may be added later. Assuming that the compost is fairly damp, no watering will be necessary. The pots may then stand side by side in a cool place, where they will be secure from frost. It is always an advantage that the eyes of early potatoes should be encouraged to start before they are planted. When left to themselves these growths come all over the tuber, but more plentifully at one end. When laid uppermost in a fairly warm place the eyes soon waken up, and to keep the growth sturdy they must be in the full light. Experts rub all the growth off excepting one as near the top of the tuber as possible, but in the case of novices I recommend two growths to be left for the systems advocated in this article. If the tubers have sprouted before potting they will soon shoot up through the soil, and may then have abundance of light, and air regulated according to the weather. As soon as the growth shows well, the pots may be taken to a moderately heated structure and watered as they require it. Should the roots appear above the soil, as is not unlikely if the house is moist, a top dressing may be given, and as the haulm gains strength the plants will bear more heat, but not too

Photo by Western Newspaper Union

**A French observation officer sending information by telephone**

much of it, or the result may be all tops and no tubers. The earliness of the crop will depend upon the treatment, and if pots are not available some similar method can be adopted with rough wooden boxes, but I prefer the former, as the whole process can be kept more thoroughly under control. The ripening-off should commence when the growth appears about completed, one sign being that it begins to flop about as though losing all energy to increase. One pot should then be tested to see what is at the bottom. Of course, the size of the tubers must not be expected to reach that of an outdoor summer crop. Successional crops may be raised as the demand needs. It is necessary to withhold water at the ripening period, or the tubers may be soft and soapy.

As to the growth in frames, the compost at the base may consist of a mixture of manure and leaves to a depth, say, of 18 inches when put in—partially sweated—when it will soon sink down. After a few days about 6 inches of nice sandy loam, in which a little lime or wood ashes has been mixed, should be laid on top and trodden down until a good level surface remains. As soon as this has become warmed planting may be undertaken. Set the tubers in rows 15 inches apart and about 9 inches apart in the rows, and as they should have sprouts on them these should be rubbed off, excepting two as near the center of the rose-end of the potato as possible. Cover with about 2 inches of soil, earthing up later on, and give all the reasonable air possible, according to the weather. When it is necessary to apply water, take the chill off, and give it in the morning of sunny days, so that it will dry away as quickly as possible. Plantings on a south border are made similar to the ordinary early summer crop. In exposed places the young growth may be protected with straw or fern in frosty times.—*London Daily Telegraph.*

## A Dwarf Elephant Discovered

There have recently arrived in England, according to the *Times*, evidences of the most important zoological discovery that has come to light since the finding of that strange beast, the Okapi, in the Congo forest some years back. This discovery proves very completely the existence of a new and hitherto unknown species of elephant, a real dwarf elephant, which in adult specimens attains no greater height than about 5 feet 6 inches to 6 feet, or about half the height at the shoulder of the ordinary African elephant. There have been rumors for some years past of an African dwarf elephant, but hitherto no real evidences of the fact have reached this country. The tusks of the two animals, which are very dark and show strong signs of wear and tear and of exposure to a moist or muddy habitat, are abnormally small. Those of the female weigh no more than 2 pounds the pair, while the tusks of the male reach 7 pounds the pair. The tusks of a well-grown African bull elephant from the region of the great central lakes often attain as much as 110 pounds apiece, or 220 pounds the pair, while in particularly fine examples a single tusk has been known to scale the enormous weight of 180 pounds. It will be seen, therefore, how puny are the tusks of the new dwarf elephant.

## Absorption and Radiation of the Solar Atmosphere

A paper by Prof. Shin Hirayama appears under this title in the Proceedings of the Tokyo Mathematico-Physical Society, second series, Vol. IX, p. 236. Utilizing observations of the radiation from different parts of the solar disc which have been made by Abbot, Prof. Hirayama computes the transmission and radiation of the solar atmosphere, on Schuster's supposition that a great part of the solar radiation comes from an absorbing and radiating layer above the photosphere. It is shown that the observations are better represented in this way than by the previous calculations of Biscoe, in which the radiation of the atmosphere was not considered. The coefficient of transmission increases gradually with the wave-length, and the radiation due to the atmosphere ranges from one-third of the whole radiation for the shorter wave-lengths to nearly one-half as the wave-length increases. Assuming the effective temperature of the sun to be 6,000 degrees Abs., it is calculated that the temperature of the photosphere is about 7,040 degrees, while that of the absorbing layer is 5,210 degrees. —*Nature.*

# Problems of Atomic Structure—VI*

## Differences Characteristic of Different Elements, and Mechanism of the Molecule

By Sir J. J. Thomson

[Concluded from Scientific American Supplement No. 2214, Page 359, June 8, 1918]

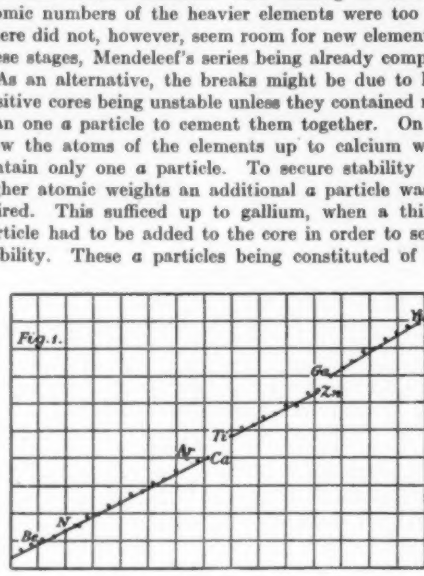
At the concluding lecture of his course on the above subject, Sir J. J. Thomson showed he had on the last occasion called attention to certain numerical relationships between the atomic weights, pointing out that if the numbers representing them were divided by four there was a tendency for the remainder to be either three or zero. With two exceptions, viz., beryllium and nitrogen, this rule held for all the elements up to calcium. This peculiarity had led to the suggestion that the positive part of the atom was built up in the first place of helium residues or $a$ particles, the $a$ particle being an atom of helium from which two electrons had been removed. In addition the nucleus contained also, he suggested, other units consisting of a system of four electrons kept together by positive charges at the center of the unit. The number of these positive charges must be either three or four, as such a system of four negative particles could not be held together by one or two positive charges. He should denote such a unit containing three positive charges as a $b$ particle and would call it an $a$ particle when it contained four positive charges. This $a$ particle with the four positive charges seemed to have a special value in the architecture of the atom. If the cores of the atoms were built of these $a$, $a$ and $b$ particles the fact that the residue obtained on dividing the atomic weight by four was either three or zero would be accounted for. Moreover, lithium with two as its atomic number, would have one free electron in its outer ring.

He would, he continued, like to say a word as to the exceptions to the rule. Beryllium had an atomic weight of just over nine, giving a remainder equal to one. It was not easy in this case to apply an independent check of the atomic weight. Nitrogen, on the other hand, was widely distributed, and there should be no difficulty as to the numerical value of its atomic weight. To be quite sure, however, he had applied the positive ray method of measuring this constant. This method was independent of the chemical properties of an element and, as it happened, nitrogen was specially well suited to the process. In the positive ray photographs the lines traced by the carbon and oxygen atoms also appeared with the nitrogen line in between them. By measuring the relative deflections of these lines the atomic weight of nitrogen was deduced in terms of the atomic weights of carbon and oxygen without the necessity of determining the constants of the apparatus. He had taken hundreds of such photographs and had measured up some of the best. These all gave nitrogen an atomic weight not differing from 14 by more than one part in 1,000. Hence there could be no doubt that nitrogen was an exception to the rule referred to and that the accepted value of the atomic weight was right. His results, moreover, disposed of another explanation of the anomaly which might have been advanced and of which a good deal was being heard nowadays in connection with other matters. It might be suggested that the apparent atomic weight of nitrogen was due to the gas, as we knew it, being a mixture of isotopes. An isotopic element was one which had chemical properties identical with another element but had a different atomic weight. As the chemical properties were identical it was not possible to separate isotopes from each other by chemical means. Hence it might be urged that nitrogen, as we knew it, was not a single substance but a mixture of two isotopes, each of which might individually obey the rule that the residue after dividing the atomic weight by four was either three or zero. Such an explanation was put out of court by the results of the positive ray determination; since with this method two isotopes would trace two different lines in the photographs.

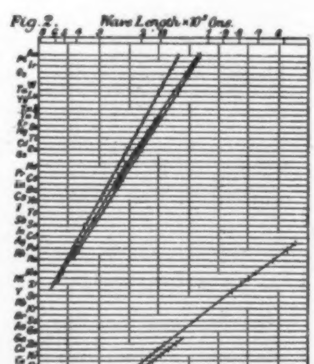
As the atomic weight of nitrogen actually was 14, we were faced with the peculiarity that the core of the nitrogen atom must have quite different characteristics from that of the atom of phosphorus, which was the element corresponding to it in the next chemical series. The problem thus raised was an interesting one. He had not been able to get positive ray photographs of beryllium.

In Fig. 1 he had plotted down the atomic weights up to yttrium against the atomic numbers, the atomic numbers being the number which denoted the position of the element when listed in order of the atomic weights. It would be seen that the resulting curves appeared to be portions of different straight lines. Taking the first section, which included all elements up to calcium, the elements plotted very well with the exception of argon. At calcium there came a break, the new line being parallel to the first section but not a continuation of it. This section extended up to gallium at which another break was evident. A possible explanation of these breaks was that certain elements were missing so that the atomic numbers of the heavier elements were too low. There did not, however, seem room for new elements at these stages, Mendeleef's series being already complete.

As an alternative, the breaks might be due to large positive cores being unstable unless they contained more than one $a$ particle to cement them together. On this view the atoms of the elements up to calcium would contain only one $a$ particle. To secure stability with higher atomic weights an additional $a$ particle was required. This sufficed up to gallium, when a third $a$ particle had to be added to the core in order to secure stability. These $a$ particles being constituted of four




electrons held together by four positive charges were electrically neutral. The presence of an additional one would not, therefore, affect the outer ring of electrons or the position of an element in Mendeleef's series. It would, however, raise the atomic weight by four units. On these lines the breaks shown in Fig. 1 would be accounted for by the supposition that at a certain stage stability required that the atom should contain two $a$ particles in place of one, and at a later stage three $a$ particles instead of two.




One or two peculiarities in Fig. 1 seemed to point to some phenomenon of the above kind. The atomic weight of argon appeared to be too high. It ought to come before potassium instead of after it, with an atomic weight of 36 instead of 40. This fact suggested the view that argon lying near the critical value was unstable with only one $a$ particle in its core and accordingly had two. This addition would raise its atomic weight by four. Another point of importance in this connection was that the only known example of radio-activity in elements of small atomic weight was provided by potassium. That potassium was radio-active had been first shown by Campbell. This radio-activity might, on the view put forward above, be associated with the position of this element in the diagram, Fig. 1, where it appeared to be hovering on the verge of instability, being close to the first break where the Mendeleef series changed from the short series comprising eight elements to the long series having 16. The second break, it would be noticed, occurred in the middle of this long series at gallium. This suggested that the properties of the atom might depend not merely on the number of free electrons but also to some extent on the arrangement of the positive part, the properties being different according as the core contained an even or an odd number of the $a$ particles. Taking the series Li, Na, K, Cu, Rb. etc., the three first elements would all, on the view put forward, have one $a$ particle, which was an odd number. Copper in the same series would contain two $a$ particles, which was an even number and it was not until we got to Rb with three $a$ particles that we got back to the properties of sodium. The case was similar with the series F, Cl, Mn, and Br, so that whether a core contained an even or an odd number of the $a$ particles might have a considerable influence on its chemical properties.

Mr. Moseley had studied the frequency of the characteristic radiation emitted by different elements when exposed to Röntgen rays. He found that if he plotted the square root of the frequency of this radiation against the atomic number of the element the resulting curves were straight lines (see Fig. 2). This was, Sir Joseph said, exactly the relationship between the two which would hold if the cores of the atoms were built up in the fashion described at the commencement of the lecture. It could be proved that, with that constitution, the highest frequency emitted would be in accordance with the following formula:

$$\text{Atomic number}-s=\sqrt{\text{Frequency}}$$

Mr. Moseley's results indicated that $s$ was an absolute constant. On the speaker's theory the value of $s$ depended upon the absolute number of electrons in the ring nearest the center of the atom. If this number were constant, Moseley's result followed exactly. If the first ring were in all cases built up of electrons at the corners of a twisted cube Moseley's result would hold, but there should be a difference in $s$ if in any case the configuration of these electrons were represented by a dodecahedron. Such a difference in the arrangement would cause a slight, but only slight, disturbance in Moseley's law, and further investigation was required.

Moseley's results had reference to vibrations of the innermost core of electrons, but some of the results observed when the electrons of the outer shell were set in vibration were sufficiently striking as to lead to the expectation that they would be capable of affording us some information as to the characteristics of this outer ring. These vibrations were excited by rays incapable of penetrating to the inner core. Thus, the lecturer showed that on bombarding different substances with soft cathode rays the glow produced was of a very characteristic kind. A noteworthy point, he said, was that quite slight changes in the chemical composition might make great differences in the color and intensity of the glow, and he thought accordingly that a study of this phenomenon might yield a good deal of information as to the nature of the outer layer of electrons. Sir William Crookes, indeed, had applied the method to sort out rare earths, tracing thereby the results of his fractionations. The same study might also give data as to how the atoms were linked together in the molecule.

In the next place he would like to say a word as to the enormous importance of extreme accuracy in the determination of atomic weights. These atomic weights could be regarded roughly as whole numbers, but the interesting point lay in the small differences actually found between the atomic weights and the nearest interger. From the theoretical standpoint these differences were of enormous importance in connection with the views entertained as to the nature of gravity and as to the energy available within the atom. We regarded the fact that the atomic weights were so nearly whole numbers as due to the atoms being built up of positive units equivalent to that of the hydrogen atom. The atom was thus built up by putting together a number of these equal units so as to form a sort of chemical compound of a very intimate kind.

Enormous labor had been spent, and special laboratories built for the express purpose of determining whether chemical combination was accompanied by a change of weight. The conclusion at present accepted was that no such change had yet been detected.

The possibility of detecting a change of this kind was more promising with radio-active bodies as so much more energy was developed in their changes than in chemical reactions. The attempt was first made at Cambridge, where the speaker had compared by pendulum experiments the the ratio of mass to weight in the case of rad-

*Reported in *Engineering*.

ium. The result showed that this ratio had the normal value to one part in a few thousand. A further experiment with a uranium compound gave the normal result to an accuracy of one part in 30,000 to 40,000, and quite recently Zeeman had obtained the same result with a limit of error not exceeding one part in three or four million. If any such change of weight were to be detected it would be when we came to the inner cores of the atom where the union between the constituents was far more intimate than it was in any chemical compound. Any change of weight due to the formation of this core would show itself as a difference between the atomic weight and the nearest whole number.

Looking at the small differences between the atomic weights and integers it was evident that extreme accuracy was required in order to form any reliable conclusions as to possible changes of weight consequent on the assembling together of the atomic cores. In these questions the natural unit to use was the atomic weight of hydrogen and not that of oxygen which was the basis used by chemists, and on which the atomic weight of hydrogen came out as 1.008.

Taking all the elements up to chromium (with high atomic weights differences from integral values were difficult to detect), it would be seen that in the vast majority of cases the atomic weight (on the hydrogen basis) was a little less than the nearest integer, and further that reckoned as a percentage of the atomic weight this difference was almost exactly the same. This result would naturally follow if the atoms were multiples of some standard substance. If we took oxygen as 16 ($H = 1.008$) the atomic weights were more nearly integral than with $H = 1$. In the vast majority of cases (Be. Mg. Si. and chlorine were exceptions), the atomic weight was less than the nearest integer. The most conspicuous exception was afforded by chlorine, and the speaker had accordingly examined this gas by the positive ray method and obtained thereby the normal value for the atomic weight.

The fact that with the above four exceptions the atomic weights were less than integers was from one point of view of almost critical importance. Theory indicated that changes of mass should be produced by changes of energy. There was a change of energy when chemical combinations occurred, and the corresponding change of weight could be calculated. This change in weight would be, in short, such that if it were set moving with the velocity of light its kinetic energy would be equal to the energy developed in the reaction. Calculation showed that this change in weight was too infinitesimal for measurement.

If, however, the hydrogen unit in the interior of atoms had the value 1 instead of 1.008, we had here a change of weight of nearly one per cent, due to energy liberated in bringing up the units together. This change in weight seemed small, but calculation showed that the energy which must have been liberated was very great. The formation of one gramme of the substance would, in fact, be responsible for the liberation of energy equal to 45,000,000 tonnes-meters.

The fact that the atomic weights of beryllium, magnesium and chlorine were more than integers indicated that energy of this order had been stored up in bringing together the units forming the cores of their atoms.

As already stated the proportion by which the atomic weight differed from an integer was the same for very many elements. This fact told one thing about how the core had been built up. Assuming the core to be built up of $\alpha$ particles, these particles had first to be constructed by getting together four positive charges; and the core was then built up of the units thus formed. If the greater part of the energy were due to the attractions of these different sets for each other, its amount would be proportional to the square of the number of sets. As stated, however, the energy liberated was simply proportional to the number of sets. Hence most of the work done was in forming the $\alpha$ particles, and the energy liberated on subsequently bringing these $\alpha$ particles together was but a small fraction of this.

If we could get to know the arrangement of things in the cores of the atoms we should also get light on the nature of gravity. Hence the study of the atomic weights was a matter of the greatest importance. It had, therefore, attracted a good deal of attention and much more was known now than 20 years ago. The speaker thought it would be desirable after determining these atomic weights by the usual chemical methods to check them by the positive ray method so as to exclude errors due to the existence of possible isotopes.

There were great openings for further research on these matters. All, however, had had to be put aside for work of more pressing importance. These lectures had suffered accordingly, Cambridge was engaged on war work, and it had not been possible to transport to London apparatus he would otherwise have exhibited, nor had it been possible for him to give as much time to the preparation of the lectures as he should have otherwise desired.

## Colloids and Chemical Industry*

Colloid chemistry, in its widest sense, deals with chemical processes which occur in the immediate neighborhood of surfaces—that is, chemical effects which are brought about as a result of capillary and electrocapillary forces. Such effects are necessarily limited to a small range, the thickness of the capillary layer being of the order $10^{-6}$ to $10^{-7}$ cm. It is obvious that these effects can become of importance only if the surface area itself is very large. Under ordinary conditions, in which two fluid masses in bulk are separated by a definite surface—as in distribution phenomena—the capillary effects are too small to be observed. To magnify the effect it is usually necessary to realize a state of affairs in which one phase is distributed in a state of fine subdivision or "dispersed" through the other phase or medium. In these circumstances the total interfacial area is enormously great. We find such conditions in the case of fine suspensions (diameter of particle $10^{-4}$ cm. approx.), emulsions (diameter of particle $10^{-5}$, and colloid solutions (diameter of particle $10^{-6}$ cm. approx.). Colloidal solutions are systems in which the solute individuals or *sols*, though apparently soluble, have not broken down to the molecular limit, but consist instead of aggregates, composed roughly of several hundred molecules or atoms. Such soluble aggregates or *sols* will not diffuse through membranes (as Graham showed in his original work on the colloidal state), and thus differ markedly from the behavior of dissolved crystalloids, *e. g.* salts.

The most fundamental problem in connection with such disperse systems is the problem of their stability. It is evident that uniformity in size of the particles plays an important part in this connection, as do also the electric charge and the Brownian movement which each particle possesses. The methods whereby the equilibrium is disturbed are equally remarkable and characteristic. A very minute amount of electrolyte added to a stable colloidal solution may bring about complete precipitation or flocculation of the *sol*, the *sol* separating out in a gelatinous form known as a *gel*. In some cases, and possibly in all—though this is a disputed point—such precipitation may be reversed. A closely allied phenomenon is that known as "peptisation," in which a substance, normally insoluble in a solvent, may be made to dissolve by the addition of a peptiser. This is illustrated by the stabilizing or protective effect produced by a small quantity of gelatine (itself a colloid) upon solutions of colloidal metals, and also by the well known phenomenon met with in the case of the hydroxides of zinc and aluminium which "dissolve" in excess alkali. Experiment has shown that the alkali may be dialyzed away and the peptized colloidal hydroxide reprecipitated. Such phenomena depend essentially upon selective absorption or surface condensation of certain parts of the peptizer (usually the hydroxyl ion), upon the suspension or colloid. Gibbs showed, many years ago, that, as a thermodynamic necessity, any substance (solute) which lowers the surface tension of the solvent is positively adsorbed at the surface—that is, the concentration of the solute is greater in the surface layer than it is in the bulk of the solution. This phenomenon lies at the basis of many technical operations, such as dyeing and tanning, though, of course, other effects of an irreversible character enter later.

Another important surface phenomenon is that known as electrical endosmose. If a liquid be divided into two parts by means of a porous partition or membrane, and an electromotive force be applied across the partition, the liquid will be found to pass through the membrane, the direction of motion depending upon the electrical state of the partition in relation to the liquid and its constituents. By a suitable choice of membrane and solution certain constituents may be separated from others, *e. g.* crystalloids from colloids, or certain colloids may be precipitated and others left in solution.

Surface effects, the realization of colloid equilibrium, electrical neutralization, preferential adsorption, peptization, colloid precipitation, imbibition or swelling of *gels*, electrical endosmose, and other phenomena of a similar nature might at first sight appear to have little significance for industrial operations and processes, although their importance has already been recognized to a certain extent in other directions, *e. g.* in agricultural processes (quality of soils, retention of salts, emulsions for crop spraying, etc.), in geological formations, and in biological problems (cell contents, nature and permeability of cell-walls, distribution of electrolytes, blood serum, coagulation of proteins, enzyme action, etc.). That colloidal phenomena enter into numerous technical processes may be demonstrated by a brief enumeration of some industrial operations which depend fundamentally upon what we may call the principles of colloid chemistry.

We have already instanced dyeing and tanning. We find further that colloid chemistry plays a fundamental part in certain stages of soap manufacture; in washing and scouring processes, in connection with textile fabrics, hides, skins, and in fur dressing; in mercerization and finishing; in the manufacture of photographic materials; in the treatment of cellulose and wood pulp in paper manufacture; in paper sizing and coloring (carbon and other copying papers); in the treatment of gums, gelatine, albumin, starch, tragacanth, and adhesive materials generally; in the clarification of wines; in filtration processes, treatment of sewage, river sludge, and the function of charcoal purifiers; in the de-emulsification of water in steam turbines; in the preparation of medicinal emulsions; in the manufacture of margarine and other foodstuffs; in brewing and fermentation industries; in catalytic reactions, such as the hydrogenation process; in chemical analysis, electro-analysis, and electro-deposition processes; in the coagulation of rubber latex and in vulcanization; in the manufacture of celluloid and celluloid products; in the flotation process of ore separation; in the manufacture and setting of cements, plaster, and mortar; in the preservation of building materials; in the manufacture of ruby glass, opaque glass, and enamel; and in the application of electrical endosmose to peat drying and the preparation of pure colloids for medicinal purposes.

The above rather heterogeneous list—by no means exhaustive—will give some idea of the variety and extent and consequent importance of colloid chemistry for the chemical manufacturer. It is an urgent matter that the great significance of this branch of chemistry should be recognized by all interested in the progress of chemical industry.

In the first report of the British Association Committee on Colloid Chemistry and its Industrial Applications, now before us, several of the processes mentioned above are discussed. The committee has aimed at compiling information regarding the advances which have been made in colloid chemistry itself and in its applications to industrial processes, with the object, in the first place, of making such information as widely available as possible, and, in the second, of emphasizing the need for much greater attention being paid to this wide, but hitherto neglected, branch of chemistry. Each subject has been treated by an expert, so that the selection and presentation of material may be regarded as authoritative. It is evident that at the present time there is a very considerable "lag" between scientific knowledge in this field and industrial practice. The result is that the majority of working processes are largely empirical, their mechanism obscure, and the probability of improvement consequently small. This is obviously an extremely unsatisfactory state of affairs. The remedy lies, of course, in the vigorous prosecution of research over the entire range of colloid chemistry in the research laboratories of manufacturers and in the chemical departments of our universities.—W. C. McC. Lewis, in *Nature*.

* First Report of the British Association Committee on Colloid Chemistry and its Industrial Applications. (1917.)

## Acid-Proof Nickel-Copper-Tungsten-Iron Alloys

Looking for an alloy which would not be attacked by sulphuric acid, the author found that the Ni was dissolved from Ni-Cu alloys and that the electrolytic corrosion of voltaic couples of the two metals did not afford any clue, as is sometimes assumed, as to the corrosion of alloys of the two metals. An alloy of Ni with 20 per cent of tungsten resisted sulphuric acid, but was difficult to machine. He then prepared alloys of Ni or Cu with W in electric furnaces. Of the Cu-W alloys that with 4.98 per cent W proved very resistant to acid and very strong, giving also high elongation: the electric resistance was higher than that of constantan. Very good results were also obtained with several ternary Ni-Cu-W alloys. These results were further improved by adding iron to the alloys, so that technically ferro-tungsten could serve as material. With increasing Fe content (up to 4 per cent), the resistance to acid and the strength increased. For hot concentrated acid a quaternary alloy with 43.65 Cu, 3.7 W, and 1.87 Fe seemed best; this alloy was strong, could be machined, and had a high electric resistance. For cold acid, alloys should be rich in Cu and relatively so also in W and Fe. Some of the alloys, however, tended to separate in layers. Attempts to improve German silver by the addition of W failed; Zn and W would not alloy, and all the quaternary Cu-Ni-Zn-W alloys were attacked by acid.—Note in *Sci. Arts*, on an article by R. Irmann in *Metall u. Erz*.

# Testing Screw Threads*

## Optical Projection Apparatus for Securing Accuracy

To a very considerable extent the optical examination and measurement of screw threads on gauges or other work requiring great accuracy is now being conducted by the projection of a magnified image of the thread on to a screen. Compared with the former optical method of performing the work, under which the thread was examined beneath a microscope fitted with a cross-wire in the eye-piece and micrometer slides, the optical projection method has several advantages. Its accuracy is not less—although neither optical method can in general yield results as accurate as those obtained by mechanical measurements—while it is, perhaps, simpler and quicker and requires less skill to carry out in practice. Its chief advantage, however, resides in the fact that it permits the magnified image of the screw-thread to be examined by direct comparison with a correspondingly enlarged outline drawing of the thread-profile. Such a direct comparison is, of course, not impossible in the microscope method, for the eye-piece, in addition to carrying the crosswire, could be made to contain an outline of the thread profile. But in this case the outline would have to be drawn accurately to the natural size, whereas in the projection method it is drawn from 50 to 100 times the natural size. It is to be noted, too, that the optical projection method is not primarily confined to the examination of *screw* gauges, but can be readily extended to the examination of flat plate *form or profile* gauges.

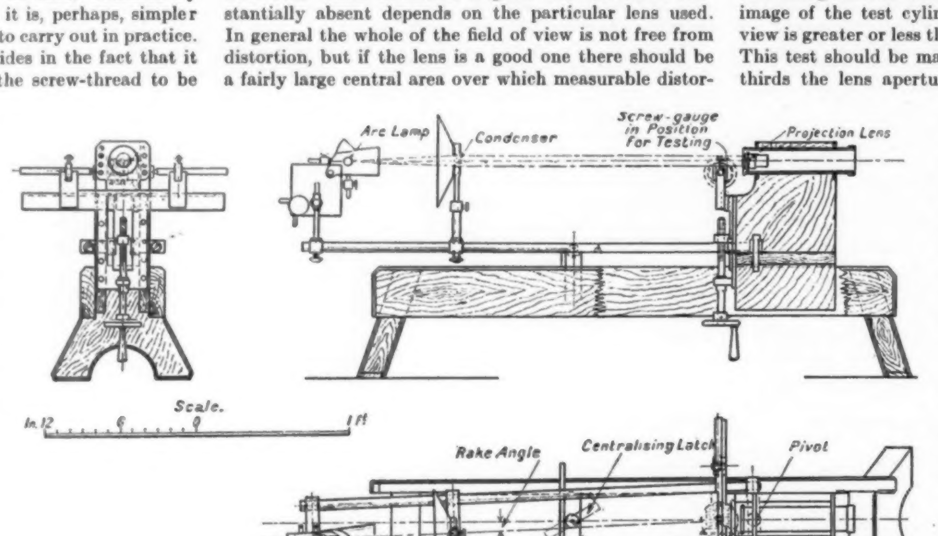



**Fig. 1. Projection apparatus for testing screw threads**

Information concerning the construction and operation of optical apparatus for testing gauges is to be found in the recently issued new edition of the National Physical Laboratory's "Notes on Screw Gauges,"[1] and from this brochure we have taken the liberty of extracting the information on which this article is based.

One of the most important requirements of a satisfactory optical projection apparatus for the purpose here being dealt with is that the optical system used should be such as to give an undistorted image over the area of field employed. Other requirements are that the beam of light should be substantially parallel and that the definition of the image should be sharp. These requirements impose a limit on the magnification with which it is possible in practice to work. For ordinary screw gauges a fifty-times magnification is recommended, but for small screws a magnification of 100 times can be used.

A simple projection apparatus for screw threads is

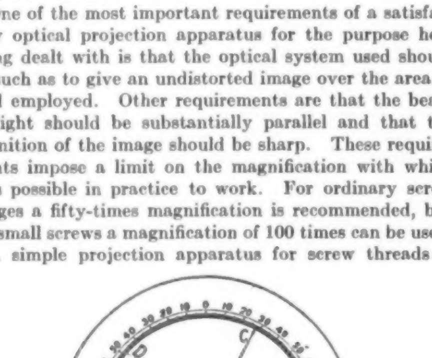



**Fig. 2**

illustrated in Fig. 1. The gauge is supported between centers with its axis accurately at right angles to the axis of the projection lens, and is carried on slides, so that it may be moved either vertically or horizontally in a plane parallel with the plane of the screen. The lens is mounted with its axis horizontal and can be moved in the direction of its axis, so as to permit the image to be focussed on the screen. For most purposes a lens having a focal length of from 2 to 4 inches may be used. Thus a lens of 3 inch focal length will give sufficient clearance between its rear face and the nearest point of the gauge to permit screws up to 4 inches in diameter to be examined. To obtain a magnification of 50 times with such a lens the screen would need to be about 13 feet away from the gauge, so that in all a room at least 15 feet in length would be required for the apparatus. Care is necessary in the selection of the lens. It should be characterized by a large aperture and very good central definition. Dallmeyer kinematograph lenses, high-class low-power microscope objective lenses and some high-class photographic lenses have been found suitable. The field of view obtained is limited by the diameter of the face of the lens turned towards the gauge, while the area of this field over which definition is good and distortion substantially absent depends on the particular lens used. In general the whole of the field of view is not free from distortion, but if the lens is a good one there should be a fairly large central area over which measurable distortion is absent. A lens of ¾-inch aperture magnifying 50 times will illuminate a circular area on the screen of about 3 feet diameter. If the lens is a good one, a central area about 20 inches in diameter should be sufficiently free from distortion to be usable, while if the lens is not so good, a central area of only 8 inches or 10 inches in diameter may be sufficiently true. In the former case the image of about four threads and in the latter of one or two threads of a screw having 10 threads to the inch will be cast within the practically distortion-free area.

The source of light in the apparatus illustrated is a small arc lamp of the right-angle pattern, taking a current of about 4 ampères. It is important that it should be of a type that maintains its crater in a fixed position. The condenser is an achromatic lens. A binocular object glass is found to be suitable for it. The distance between the gauge and the condenser should not be less than 18 inches so that want of parallelism in the beam may be readily observed. In the apparatus illustrated the lamp and condenser are mounted on a framework of rods, which can be turned about a vertical pivot beneath the projection lens.

The screen may be a three-ply board surfaced with Bristol board, on which the true thread forms for various pitches are drawn to the predetermined magnification. In this case the screen is held against the wall and adjusted by hand. It is better, however, to use a screen of the form shown in Fig. 2, namely, one mounted in suitable slides, so that it is free to be moved vertically and horizontally and to be rotated.

The apparatus is erected on a table at a distance from the screen which is such as to give, in the first instance, the required magnification approximately. The crater of the arc lamp and the centre of the condensing lens are placed exactly on the axis of the projection lens—a centralizing latch is provided for this purpose—and the condenser is moved away from or toward the lamp until the beam of light is observed to be parallel. A parallel cylinder, of diameter about ½ to ¾ the aperture of the projection lens, is then placed between the gauge-supporting centres, and its image focussed on the screen. The whole apparatus has then to be moved away from or toward the screen until the size of the image, as measured at the *centre* of the field, is the required number of times the known size of the cylinder. If the widths of the image at two points equidistant from the center of the field to the right and left are unequal—they need not be examined at this stage for equality with the width of the image at the centre of the field—then the axis of the instrument is not at right angles to the plane of the screen, and has accordingly to be adjusted. Both edges of the cylinder should come into focus simultaneously; if they do not, either the screen is not truly vertical or the axis of the instrument is not truly horizontal. If when one edge of the test cylinder is placed so that its image, in sharp focus, falls at the centre of the field of view, and if the image of the other edge of the cylinder is then materially out of focus, the lens is not suitable for the work. The distortion of the lens can be studied by examining whether and to what extent the width of the image of the test cylinder at the centre of the field of view is greater or less than the width towards either edge. This test should be made with a cylinder of about two-thirds the lens aperture in diameter, and also with a cylinder of much smaller diameter.

On replacing the cylinder by the screw gauge to be examined the centralizing catch is freed, and the frame-work turned through a small angle so that the beam of light is directed along the rake of the screw. This will be correctly secured if both flanks of the thread at the center of the field are equally well defined on the screen, and in focus. It is sometimes difficult to determine when this condition is reached. In such a case the angle through which to turn the frame-work can be calculated from the approximate formula for the rake of a screw thread, namely, rake (in degrees) $= 18/nE$, where $n$ is the number of threads per inch and E the effective diameter of the screw.

The screen arrangement, illustrated in Fig. 2, consists of a white disc A of, say, about 8 inches in diameter, recessed flush into a board B, within the hole in which is it free to rotate. The disc is marked with a thread-form diagram, with a central line C C to correspond with the cross-wire of a measuring microscope, and with three parallel lines, one touching the crests, one touching the roots, and one bisecting the threads of the outline. To measure the angle of the thread and its squareness, the line C C is first set to the zero graduation of the protractor scale marked on the carrier B. By moving the shadow up or down, and by operating the screw E so as to tilt the screen if necessary, the line D D is made just to touch the crests of the thread. The disc is then rotated, and the shadow moved to the right or left as

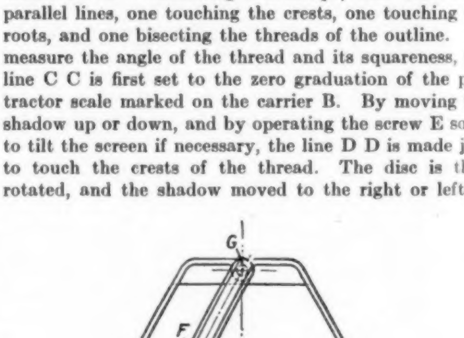



**Fig. 3**

required, so that the angle which each flank of the thread makes with the vertical may be measured by the line C C against the protractor.

An alternative form of protractor is illustrated in Fig. 3. In this a metal straight edge F, pivoted to a metal sector frame at G, takes the place of the line C C in Fig. 2. This straight-edge stands at some small distance above the white background, on to which it throws a shadow. The edges of this shadow can be made to coincide with the flanks of the thread shadow with a greater degree of accuracy than is obtainable with a line. For the same reason, in place of the horizontal line H H on the white background, there may, with advantage, be substituted

(*Concluded on page 381*)

*From *The Engineer*.

[1] "Notes on Screw Gauges" (enlarged issue), by the Staff of the Gauge Testing Department, National Physical Laboratory.

## Occultation of a Star by Saturn's Rings

In the *Journal of the British Astronomical Association* for May, 1917, is given the account of the observation of a unique phenomenon by two observers, Messrs. M. A. Ainslie and J. Knight. On the night of February 9, 1917, a star of about the seventh magnitude was watched while the Ring A and the Cassini Division of Saturn's Rings passed by it. Its apparent path through the rings is shown in the accompanying diagram which is reproduced from a larger diagram in the *Journal*. Both observers state that the star could be seen all the time it was watched except during moments of bad seeing. Its light when behind Ring A was diminished to about one quarter of its normal brightness, and its color was apparently changed from golden yellow to cream white. While seen through the Cassini division it was almost as bright as when entirely clear of the planet. As it was passing the outer parts of Ring A there were two moments, lasting half a second or more, when the star brightened but did not regain its full brightness; these were probably when the star passed the Encke Division and possibly another minor division in the ring outside of that. The recovery of brightness of the star as it passed clear of the ring was not instantaneous but occupied perhaps a half second of time.

The interest which attaches to these observations is chiefly on account of what they indicate as to the structure of Saturn's outer ring. Estimating the apparent diameter of a star disk to be one thousandth of a second of arc (which is a large estimate), the distance subtended by this arc at the distance of Saturn would be about four miles. The particles of which Ring A is composed must be much less than four miles in diameter or there would have been many complete disappearances of the star. Here is thus a new confirmation of the theoretical prediction that the rings of Saturn are composed of small separate particles. The particles are small enough and far enough separated to allow about one-fourth of a star's light to pass through the thickness of the ring, which augmented by the inclined line of passage may be 100 miles or more.—Notes in *Popular Astronomy*.

## Submersible Cargo Vessels*

Cargo carrying by crewless barges has been very considerably developed during the last twelve months. For cross-Channel traffic most shipping men will be familiar with the new type of barge that has carried such huge supplies of ammunition to our Army overseas. An excellent double-page picture of one of these vessels was given in the *Illustrated London News* a short time ago —that of a low and short tug with little freeboard or top hamper towing one of these big, almost submerged, cargo crates. The waves are shown washing across her watertight deck, which even at a short distance would not be visible to any enemy submarine, and would present a very difficult target. The tug once sunk or put out of action would, of course, leave the cargo barge hopelessly at the enemy's mercy. She would remain just awash on the surface to be torpedoed or shelled at leisure, and the only saving would be that of the men who in other case might have been on board.

Now, the project put forward by Professor C. V. Boys, F. R. S., Dr. Hele-Shaw, F. R. S., and their associates is simply that of similarly-built vessels, with this important difference—that theirs, instead of being towed awash and visible on the surface, would be towed submerged at, say, 30 to 40 ft., so as to be completely nvisible, not only from above, but to any submerged U-boat. Vessels under water are invisible to each other within a few feet, and in case of collision there is every probability of the submarine receiving as much damage as the submerged cargo vessel, which would be built in watertight compartments. It is a comparatively small matter of difference in construction or equipment from those now so long in successful service, but the difference is all important. At the present crisis the striking and essential inducement for building them is that owing to extreme simplicity of design they can be turned out with a rapidity absolutely unapproachable in comparison with any other class of cargo vessels.

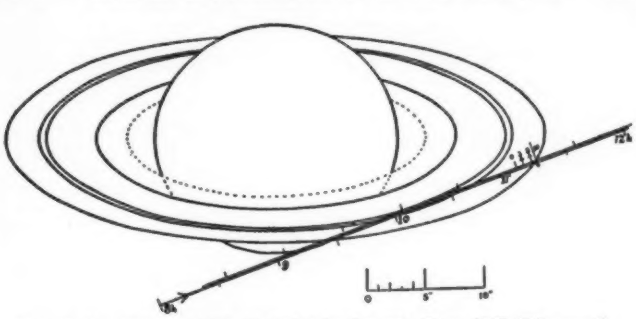
Briefly, the features of the proposition are as follows: With the object of carrying cargo immune from U-boat attacks across the Channel, the North Sea, and, possibly, the Atlantic, it is proposed to construct steel or concrete vessels of desirable shape, and designs for 1,500-ton and 5,000-ton vessels have already been prepared. These vessels are to be loaded with cargo and adjusted at the port of departure so as to sink the crafts to just above the surface level. They would be fitted with vertical and (if necessary) horizontal rudders controlled from the tug. After leaving port the tow would be completely submerged.

*The Shipping World.

The tugs would be well armed, provided with paravanes for mine sweeping, fitted with wireless apparatus, and in order to equalize the strain on the towing hawser would be provided with steam towing machines, which have proved so serviceable for heavy towing.

In the event of attack by U-boat, on her appearance, the tug would slip the towing cable immediately, and presenting a small target and being well armed would have a good chance of escape from the attacker, and would instantly send out a call to be picked up by the nearest patrol boat or destroyer, as well as by aircraft stations on the coast.

From the time of being cast off the barge would come under the control of the automatic stabilizer, a patented invention of Professor C. V. Boys, F. R. S., the principle being the displacement of the necessary amount of water by means of the movements of the depth indicator. The barge would then descend to any prearranged depth, say, 30 or 40 ft., so as to be clear of passing vessels. As torpedoes are set to travel about 8 or 10 ft. below the surface, the submerged vessel would be out of their range, so that if discharged without being reset they would pass harmlessly above her. The action of the stabilizer could be regulated so that the vessel would remain at the desired depth if necessary for several days, and then caused either to rise to the surface or sink and rest on the bottom, as may be previously decided, and in the latter case only where the depth of water is not too great.



Occultation of the star BD +21°1714 by Saturn's Ring, 1917, February 9.

Attached to the cast-off submerged vessel would be two buoys, which would be submerged with the vessel, so that she should leave no sign on the surface of her location, and then by a clockwork attachment these buoys would be released and caused to rise to the surface after any predetermined number of hours. They would be provided with acetylene lights. One would be attached to the submerged vessel, the other to a light, strong line fastened to the end of the cable to facilitate the recovery of the barge.

We already have an example going some way to prove the practicability of this system in the historic voyage of what might almost be considered a submarine vessel. She was completely awash while being towed from Egypt, was abandoned in the Bay of Biscay, picked up again after two months and brought safely into the Thames—the *Cleopatra* containing Cleopatra's Needle.

Among recent instances of heavy towing is that of the *Maumee*, a 14,000-ton oil tanker, which was towed by the *Prometheus*, 12,500 tons, from San Francisco to New York in twenty-six days, at an average speed of 9 knots, while the *Narragansett* made many successful and profitable voyages across the Atlantic towing an immense oil tanker.

Some of the advantages claimed for this system are:

(1) The extreme simplicity and cheapness of construction of the submerged cargo vessel, and the rapidity with which these vessels would be turned out once the best designs were selected and standardized.

(2) When an ordinary cargo steamer is torpedoed she sinks with her entire cargo, whereas in the case of the sinking of the tug the submarine cargo vessel would remain concealed, to be recovered by any of the vessels of the Fleet possessing control of the surface sea.

(3) The economical employment of power and crews on the tug, one tug being capable of handling relays of barges from either side of the Channel.

(4) In view of recent instances of heavy towing, such as that of the *Maumee* at the rate of 9 knots by a vessel whose normal speed was 14.67, it may be as well to consider whether it would not be worth a similar reduction of speed for powerful trans-Atlantic steamers to tow submerged cargo vessels of practically their own tonnage, completely secure against U-boat attacks. With the present convoy system fast steamers have constantly to be slowed down more than this to the speed of the slowest ship of the convoy.

When this novel proposition was first entertained, there appeared many difficulties in the way, such as, for instance, the complete control of the submerged vessel from the tug, the production of power on the towed vessel, the delicate question of stability, and, more especially, the problem of maintaining the vessel in equilibrium after she had been abandoned.

Those who have been at work on the problem and a number of experts who have studied it now consider that these obstacles have all been overcome by methods which are the subject matter of several patents, and that it only requires practical trials to arrive at a type of boat which can be standardized so as to ensure the rapid production of cargo vessels to replace our losses.

The only serious objection to the scheme that has been brought forward is well and conclusively answered by its advocates—*viz.*, supposing the U-boats sink the armed tug, would she not be able to destroy the cargo vessel by means of a torpedo or high explosive depth charge? The enemy will have no knowledge of what the length of the towing cable is or to what depth the barge is submerged, and, finally, at what angle the tug sets the tiller of the submerged vessel at the instant she casts her off. Supposing she is being towed at, say, 8 knots and her helm were set hard aport or starboard, she would travel in a curve in either direction for three-quarters of a mile to a mile before coming to rest, and there would be nothing to indicate her location until her automatic buoys rose to the surface twelve hours after, or at whatever time they were set to do so.

## Testing Screw Threads

(Concluded from page 380)

the edge of the shadow cast by a second straight-edge fixed horizontally across the frame below the level of the swinging straight-edge.

The use of the above described optical projection apparatus for measuring errors in the pitch of a screw thread is not recommended, for very slight errors in the focussing of the image may give rise to appreciable errors in the pitch measurement, particularly in the case of threads with a coarse rake.

If the carriage in which the gauge is supported is fitted with micrometer movements, the usefulness of the instrument is widely extended. A form of projection apparatus on these lines has recently been developed at the Laboratory. It differs from that described above, not only in the provision of micrometer movements on the gauge carrier, but in the fact that the projection is made vertically. Thus the gauge, with the lamp below it, is mounted on centers just above a table. The beam of light is all but vertical, and is directed back to the table by a horizontal mirror placed some distance above the gauge. Thus the gauge and the image are brought close together, so that they may be simultaneously under the control of the operator.

For some purposes, as, for example, the examination of form gauges, an exceptionally large field of view free from distortion is desired. This can be obtained by using two suitable projection lenses in combination. Apparatus of this type capable of giving a circular area on the screen of 8 feet in diameter at a magnification of 50 times has been designed at the Laboratory with good results as to definition and freedom from distortion.

## Spontaneous Ignition of Haystacks

Since about 1913, haystack fires have been much more frequent than before in Switzerland, probably because modern mechanical appliances enable hay making and stacking to be completed much more rapidly than was formerly the case and in consequence the hay often has less opportunity to dry. The author refers to the work of various investigators, viz., Miehe, Ranke, Schenk and Boekhout and de Fries, on the heating of stacks, and concludes therefrom that a rise in temperature to 40 degrees C. may occur owing to respiration of the surviving cells and also to intramolecular respiration; the activities of microorganisms account for further heating to about 70 degrees C., at which temperature a slow process of dry distillation may set in and eventually heat the stack to 300 degrees to 400 degrees C. The final products of this dry distillation are combustible gases and carbon, the latter being pyrophoric owing to the fibrous nature of the original material. Various practical precautions are enumerated; *e. g.*, care should be taken that hay is dry when stacked; stacks should be of as large cross-section as possible, and provided with means for ventilation. If the temperature in the stack should rise to 70 degrees C. the ventilation passages should be closed. Salt may be scattered over hay which has become damaged by exposure to bad weather.—Note from *J. Soc. Chem. Ind.*, on an article by E. Jordi in *Schweiz, Chem. Zeit.*

# The Coral-Reef Problem*

## And the Evidence of the Funafuti Borings

By Ernest W. Skeats, Geological Department, University of Melbourne

INTEREST in the vexed problem of the origin of atolls and barrier reefs has been again stimulated during the last few years by the writings of the two Harvard geologists, Professors W. M. Davis and R. A. Daly. Davis[1] has reinforced Darwin's view of the origin of atolls in a series of papers, by amplifying and elaborating the evidence, first recorded by Dana, in favor of the subsidence theory which is yielded by the presence of drowned valleys and embayed coasts in many of the central islands surrounded by barrier reefs.

Daly[2] elaborating and adding to the earlier work of Belt[3] and Penck,[4] among others has propounded the "glacial-control theory of coral reefs," claiming that the Pleistocene glaciation by means of Polar ice caps locked up so much water that a lowering of level of the tropical seas of 50-100 meters occurred. The lowering of temperature is pictured as killing most of the corals, while prolonged abraison of oceanic islands during the period of lowered sea level led to the development of wave-cut "reef platforms" which served as the foundations on which the existing atolls were built up when sea temperatures and sea level subsequently rose. The phenomena of drowned valleys and embayed coasts are by Daly attributed to rise of sea level and not to subsidence of the land.

Davis, in common with others, admits some change of sea level due to glaciation and subsequent melting of polar ice caps, but doubts the quantitative significance which Daly attributes to it, and indeed, regards it as of minor importance. These indirect methods of enquiry have assumed much of their importance from the circumstance that the materials and constitution of normal atolls are not commonly available for direct examination except when subsequently elevated or explored by boring.

Some fifteen to eighteen years ago the writer, while a demonstrator under the late Prof. J. W. Judd at the Royal College of Science, London, was concerned in the examination of coral limestones from upraised coral islands[5] and also made a large number of the analyses of the materials of the cores from the borings into the atoll of Funafuti. For some years he was handling the sliced cores of the latter bore and has examined most of the rock sections so ably described by Professor Cullis in the mineralogical part of the Funafuti report.[6] The writer's familiarity with the bore cores from Funafuti and his experience with limestones from upraised coral islands constitute his justification for contributing to the present discussion. The significance of the evidence made available by the publication of the very detailed and exhaustive examination of the Funafuti bores appears to have escaped many workers on coral reef problems or to have been misunderstood. This no doubt is partly to be attributed to the circumstances that the committee responsible for the work consisted of adherents of diverse views on atoll formation, and decided that the experts to whom the material was submitted should publish descriptions of the material but should draw no conclusions from the facts as to the mode of formation of the atoll. The writer believes he is correct in stating that these experts were unanimous in their views that the published descriptions supported Darwin's subsidence theory, and in fact were fairly susceptible of no other known explanation.

In this paper it is not proposed to discuss in detail the big problems raised by Prof. Davis or by Prof. Daly with the exception of three points raised by the latter, namely, the submarine profile of Funafuti, Daly's and Von Lendenfeld's views and diagrams of the development of coral atolls and the machanism of the filling of "lagoon moats."

The author is principally concerned that more serious consideration should be accorded to the positive evidence provided by the examination of the cores which is published in the Funafuti report. In a succeeding paper he proposes to recall the attention of workers in this field to the support given to the subsidence theory of atoll formation by a consideration of the apparently unrelated problem of the origin and distribution of dolomite in coral and other limestones.

Some of these facts and the legitimate conclusions which follow from them were pointed out by Professor Sollas in 1905,[7] but subsequent writers on the subject have not commented on them and apparently have not realized their significance.

### VON LENDENFELD'S AND DALY'S DIAGRAMS OF ATOLL DEVELOPMENT

Darwin's and Dana's diagrams of atoll development show a centripetal displacement of the outcrop of the reef of an atoll as the result of growth during subsidence. Von Lendenfeld[8] and Daly[9] have published diagrams showing a centrifugal displacement of the outcrop of the atoll reef during subsidence. This latter view can only be tenable on the assumption that the debris broken from the seaward face of the reef accumulates as a submarine talus *within the limits of coral growth*, and that fresh coral reef material grows upward on that talus.

On "a priori" grounds the writer would expect the following factors to determine in large measure the nature of the submarine profile of an atoll.

1. The relative rates of subsidence and of upward growth of corals.[10]
2. Rate of abrasion of the seaward face of the growing reef by wave attack.
3. Rate of lateral regrowth of corals on the seaward side of the abraded reef.

With regard to the first point, if the rates of subsidence and of upward growth are nearly equal a very steep outer wall to the reef will develop and might continue for many hundreds of feet. If this were the only factor involved a slight centripetal shift of the outcrop of the reef would be expected, for only submarine talus formed within the limits of coral growth can sustain growing corals and these will fail to reach the surface. If the rate of subsidence is less than that of upward growth of coral, the latter will be checked on reaching sea level and more material will be abraded from the seaward face, submarine talus will collect and if within the limits of coral growth upward growth of coral on the talus will commence. The accumulation of talus will be slow and whether or no corals can grow from such a talus and reach the surface will depend on the submarine slope of the island, on the relation between the rates of supply of talus material, of subsidence, and of upward growth of coral. Any considerable accumulation of submarine coral talus necessarily involves extensive abrasion of the growing reef by wave action and considered by itself will cause a centripetal shift of the position of the outer reef-face at the surface. But here the second and third factors mentioned above, which involve the relative rates of abrasion of the reef face and repair by regrowth of corals, become important. Von Lendenfeld's and Daly's diagrams can only express the facts in the case of a reef in which the rate of subsidence is much slower than that of upward growth of coral, lateral abrasion of the reef-face and its repair are rapid and a large quantity of submarine coral talus accumulates at depths less than 15 to 40 fathoms giving a foundation for the seaward and upward growth of coral. In the writer's view the combination of these factors in the history of an atoll is likely to be exceptional rather than normal and the alternative view seems more probable that usually during subsidence the upward growth of coral will occur on dead coral reef and only as the result of occasional long pauses in subsidence or prolonged periods of very slow subsidence will sufficient talus accumulate to provide a foundation for coral growth. If this latter picture of the development of an atoll is correct, on the whole a centripetal shift of the outcrop of the reef will take place.

Davis[11] has discussed this question and defined the conditions under which centripetal or centrifugal shift of the reef may possibly take place. Let us turn from these "a priori" arguments to consider the facts so far as they may be disclosed by an examination of the submarine profile of Funafuti. The diagram shown in Daly's paper is not helpful in this connection for it is generalized and the vertical scale is exaggerated three times. In the Funafuti report several cross profiles are shown on the true scale and if we limit our attention to the part within 200 fathoms of the surface (slightly greater than the depth of the main bore) it will be noticed that the cross profiles show varying slopes. It may be noted here that the soundings showed that down to 400 fathoms from the surface round most of the island the average submarine slope is about 40°, a figure which must reach if it does not exceed the upper limit for the angle of rest of submarine talus. One profile, AA in the Funafuti drawings, between the surface and a depth of 200 fathoms shows the existence of 4 very steep walls with angles up to and exceeding 70° and one of these is over three hundred feet in height and slopes at 78°. Between these steep slopes are several which are at much lower angles. These steep walls most probably represent the outer parts of the growing reef at various stages of development while the methods of formation of the intervening flatter slopes may be more debatable. The important point to note is that in the development of the Funafuti atoll we have evidence in the submarine profiles that there has been a centripetal not a centrifugal shift of the outcrop of the reef since the more deeply buried submarine steep walls representing former positions of the outer reef-face are further from the center of the island than those of more recent origin and shallower depth. An appeal to the facts as shown in the submarine profile of Funafuti indicates, therefore, that the development of that atoll is in general accordance with the diagrams of atoll growth originally published by Darwin and by Dana while Von Lendenfeld's and Daly's views and diagrams are clearly inapplicable to that particular atoll.

### THE FILLING OF THE "LAGOON-MOAT"

Daly[12] attaches much importance to the mechanism of the filling of the "lagoon-moat" in atolls. He claims that, according to the subsidence theory of atoll formation the fairly shallow and relatively flat lagoon floors which are commonly met with, imply advanced filling of the lagoon to depths of scores, hundreds or possibly thousands of meters. The filling mechanism according to Daly involves two factors, sediment and active transportation of that sediment. On this basis he claims that the lagoon floors should not be flat but should slope away from sources of supply of sediment, *i. e.*, the reef-face and the central island in the case of barrier reefs in course of development to atolls. Further he claims that the supply of sediment is quite insufficient unless in the case of all atolls a very prolonged pause has followed subsidence. He favors the view that the "lagoon-moats" really represent wave-cut surfaces or rock platforms developed by the erosion of pre-existing islands while the sea level stood lower than it does at present. Davis[13] has criticised this theory of wave-cut platforms as the support for modern atolls from the point of view among others that enormous periods of time would be necessary to develop level rock platforms in the case of islands some of which must have been 20 to 30 miles in diameter. The writer is more particularly concerned with Daly's criticism of "moat" filling previously stated above. It will be noted that although Daly quotes Darwin's view that the "moat" is slowly filled through the accumulation of detritus and shells and skeletons of organisms inside the reef, in his discussion of the mechanism of "moat" filling he ignores the activity of organisms and develops his destructive criticism on the assumption that the moat is filled entirely by means of transported and deposited sediment. While it may be granted that Daly's criticism would have some force if sediment were the only factor in moat filling, the argument is very seriously weakened if not completely vitiated by ignoring the part played by organisms. One of the best established facts in modern work on coral reefs is that any effects due to solution from lagoon waters, which was an important element in Murray's picture of the development of lagoons, is negligible in amount and quite overbalanced by organic growth and the deposition of sediment. In fact measurements show that the lagoons of many atolls are becoming shallower owing to these causes. If it can be shown that the growth and deposition of organisms within the waters of the lagoon are quantitatively much more important than the deposition of sediment the weight of Daly's destructive criticism would be removed and the ratatively flat floors of many lagoons could be attributed to the deposition of organisms. To decide between the opposing hypotheses the appeal is to the facts so far as they are known. The Funafuti

*From the *American Journal of Science*.

[1] Davis. W. M., this Journal, XXXV, pp. 173-188, 1913; ibid, XL, pp. 223-271, 1915; Bull. Am. Geogr. Soc., XLVI, pp. 561-739, 1914; Proc. Acad. Sci., Washington, pp. 146-152, March, 1915.

[2] Daly, R. A., this Journal, XXX, pp. 297-308, 1910; Proc. Amer. Acad. Sci., li, pp. 157-251, Nov. 1915; this Journal, XLI, pp. 153-186, 1916; Nat. Acad. Sci., pp. 664-670, Dec. 1916.

[3] Belt, Quart. Journ. Science, XI, p. 450, 1874.

[4] Penck, Jahr. Geogr. Ges. Münichen, VI, p. 76, 1881. Penck, Morphologie der Erdolurflasche, Stuttgart, II, p. 660, 1894.

[5] Skeats, Bull. Mus. Comp. Zool., Harvard, XLII, pp. 53-126, 1903.

[6] Funafuti report, Royal Society, London, 1904.

[7] Sollas, The Age of the Earth, T. Fisher Unwin, London, pp. 121-132.

[8] R. Von Lendenfeld, Gaca, Jahrg. 26, 196, 1890; Westermann's Monatshefte, p. 505, Jan. 1896.

[9] Op. cit., p. 247.

[10] One may neglect the case where the rate of subsidence exceeds that of upward growth of coral, since the atoll would in that case be drowned.

[11] W. M. Davis, Proc. Nat. Acad. of Sci., II, pp. 466-471, 1916.

[12] R. A. Daly, Proc. Nat. Acad. Sci., II, pp. 664-670, 1916.

[13] W. M. Davis, Bull. Am. Geogr. Soc., XLVI, p. 646, 1914.

report[14] again provides the most definite evidence of the nature of lagoon deposits. Two bores were put down from the waters of the lagoon and penetrated to depths of 1113 and 1114 feet respectively below the floor of the lagoon. The record of the deeper boring, L, shows that of the material from the first 70 feet between 80 and 95 per cent consists of loose uncemented fronds of the calcareous alga, Halimeda; below 70 feet the organisms are cemented by calcite into a white limestone and for about 20 feet consist of about one-third of Halimeda and two-thirds of foraminifera. In the lower 50 feet the rock is mainly composed of corals and foraminifera while Halimeda is scarce. It will be noted that for the first 60 feet the filling of the lagoon "moat" consists of loose fronds of Halimeda practically devoid of sediment; below this level the calcite cement, small in amount, may or may not represent recrystallized calcareous sediment. At Funafuti, therefore, it is clear that of the two factors, organisms and sediment, which have filled the lagoon "moat," the organisms are of overwhelming importance. The deposits of Halimeda in the upper part of the lagoon bore must have been accumulated fairly rapidly since from the floor of the lagoon down to a depth of 35½ feet the fronds of Halimeda were still sufficiently preserved to show the peripheral cells on decalcification. We may therefore conclude, at any rate, so far as Funafuti is concerned, that Daly's objections to the subsidence theory of atolls so far as they are based on difficulties connected with the filling of the lagoon "moat," have no weight for the predominant factor, the activity of calcareous algæ, foraminifera, corals, etc., has been completely overlooked.

*The evidence of organisms from the main Funafuti bore.*—The careful, precise and monumental work of Dr. Hinde on the materials from the borings at Funafuti provides a wealth of information on the organisms which built up that atoll. From his report, from Prof. Judd's general report on the materials, and from a personal communication from Mr. F. Chapman, now paleontologist to the National Museum, Melbourne, who sliced all the cores and examined the foraminifera, the writer selects the following statements as bearing on the question of the origin of the atoll.

Dr. Hinde reported that in the upper 180 feet of the boring, whose coral reef origin no one has questioned, about one-fifth of the organisms consisted of corals, the remainder consisting of calcareous algæ, foraminifera, and other organisms.[15] In the lower third of the boring from 750 ft. to the bottom at 1114 feet the corals form a larger proportion of the whole rock, but, even here, are considerably exceeded by the foraminiferal and fragmentary rocks. Between 600 ft. and 748 ft. coral casts are more numerous and Halimeda is abundant. He further stated that 27 genera of corals were recognized in the main boring, all of which belong to well-known reef-building forms, most of which still exist on the reef, and in the lagoon at Funafuti 35 genera of foraminifera are recognized in the main boring, of which only 7 are of importance as rock-formers, and they are still flourishing on the present reef or in the lagoon.

Prof. Judd[16] states that the corals which occur are sometimes *upright and in the position of growth*, but very frequently broken and fragmentary, this being true of all parts of the core from the top to the bottom. So far as could be made out the corals are as often "in situ" in the lower as they were in the upper parts of the core.

Mr. Chapman,[17] whose opinion is of great weight, as he did all the slicing of the cores, as well as from his position as an expert in the study of forminifera, states that "throughout the bore the corals were found in the position of growth. Pocillopora was found in the boring down to 750 feet. In the living state at Funafuti it occurs from 30-180 ft.

*Coeloria* occurs in the bore from 340-1114 ft. 6 in., and is found living at 42 ft.

*Alveolina boscii* was found in the bore down to a depth of 700 ft. In soundings it is found commonly in shallow water down to 30 fathoms, below which it is rare.

The delicate megalospheric form of *Orbitolites complanata* in its reproductive stage was found in the lowest cores of the bore in the condition in which it lives in the shallow waters of coral reefs. This material could not have tumbled down a talus bank and have been preserved loosely and intact. Moreover, it is associated in this lowest core of the boring with other evidence of shallow water conditions.

Halimeda, now living in the lagoon to the depth of about 200 ft., was found in the cores at 660 ft., proving a subsidence of nearly 400 feet."

Prof. Judd also stated that careful search was made to see if deeper water organisms mixed with those building up the reef could be detected in the cores.

[14]The Atoll of Funafuti, London, 1904, pp. 310-315.
[15]Funafuti Report, pp. 333-334.
[16]Personal communication.
[17]Op. cit., p. 173.

If any part of the bore represented material fallen from above such an admixture of shallow and deep-water forms must have occurred. Not a trace of deep-water forms was found in the lower or any other parts of the Funafuti bore. Dr. Hinde's carefully drawn up lists show that from top to bottom the same organisms occur, sometimes plants, sometimes foraminifera, sometimes corals predominating, but in the whole depth bored the same genera and species of these various groups of organisms take their part in the building up of the mass.

Moreover, as Judd states, not a trace of Orbitoides or other Tertiary fossils such as occur and have been recorded, by the writer among others[18] at Christmas Island in the Indian Ocean, and at Mango and Namuka in the Fiji group in the Pacific Ocean, was found from top to bottom of the boring.

### TEXTURAL FEATURES OF THE BORING

If any part of the boring had passed through a submarine coral talus, its coarse fragmentary condition and bedded character should have been recognizable. Prof. Judd[19] reports that "nowhere could a stratification, such as might be expected in a talus formation, be found, but only such irregular accumulation of detrital materials as takes place between and around the corals, and these appearances were presented at many points from the top to the bottom of the bore hole, whenever consolidated rock could be examined."

Dr. Cullis[20] in his valuable report draws attention to and figures remarkable stalagmitic coatings to cavities in the limestone of the bore, chiefly formed of fibrous calcite, sometimes of alternating layers of calcite and dolomite, and these are found at intervals, not only in the upper part of the bore in the case of the fibrous calcite, but also from a depth of 815 feet, down to the bottom. This material from its appearance and occurrence as a lining of cavities is strongly indicative of rapidly deposited carbonate under conditions of supersaturation such as occur under very shallow water conditions or even between high and low tide level. Dr. Cullis also draws attention to the mineralogical change from aragonite to calcite in the materials of the upper part of the bore cores. In the top cores aragonite is freely represented in the corals, and certain other organisms, and as chemically deposited carbonate. With this occurs calcite in the form of organisms and as chemically deposited material. At about 100 ft. in depth it is noted that less aragonite and more calcite are represented due partly to the deposition of calcite instead of aragonite and partly to conversion of aragonite to calcite. Below 100 ft. this mineralogical change is more noticeable, and at 150 ft. practically all aragonite has gone, the lowest depth at which it has been recognized being 220 feet.

This mineralogical change involving a change in the appearance of the rock cores, it will be noted, is gradual and not abrupt, but it is probably responsible for certain erroneous conclusions to be mentioned below. Summarizing the evidence of the organisms and of the textural features of the main boring at Funafuti it may be stated that all the organisms belong to recent forms, most of the species are still living, nearly all are forms which only live in the shallow water of the reef and lagoon, many of the reef-forming corals in all parts of the bore, including the lowest cores, occur upright, in the position of growth, no deep-water types of organisms were found and no Tertiary forms.

No evidence of coral talus and no true sign of bedding was noticeable although carefully sought for. Stalagmitic linings to cavities in the coral limestone, such as might be expected to be deposited in very shallow or tidal waters, occurred at intervals down to the bottom cores, while the change from aragonite to calcite in the material of the core involving a change in the appearance of the rock occurred gradually between the depths of 100-150 ft., and was complete at a depth of 220 ft.

It will be noted that the above facts are completely at variance with the view expressed by Agassiz[21] in his paper on the Coral Reefs of the Tropical Pacific. Therein he remarked as follows: "The boring at Funafuti reached 1114 ft. It passed at first through the modern reef rock material and below that must have, judging by analogy, penetrated either an underlying mass of Tertiary limestone or have passed through the mass of modern reef rock forming the outer talus of the atoll of Funafuti."

Prof. Daly[22] in his paper on the glacial control theory of Coral Reefs after discussing Von Lendenfeld's view of the development of an atoll by centrifugal displacement of the outcrop as sinking progresses, states: "If Von Lendenfeld's view is correct, the massive reef of a large atoll must lie unconformably upon talus of indefinite depth. Hence the Funafuti borings could not, in any case, have penetrated massive reef material in situ to a depth greater than about 45 meters." Again, at p. 218, he states: "the boring at Funafuti showed massive coral to persit to a depth of about 46 meters. Below that depth the log of the boring suggests that it passed through talus material all the way to the bottom at a depth of 340 meters. This conclusion was reached by the writer after a careful study of the Funafuti report, issued by the Royal Society of London; a subsequent inspection of a duplicate set of the core material has tended to confirm the opinion." These statements of Agassiz and Daly require some comment. The underlying mass of Tertiary limestone beneath a shallow reef, pictured by Agassiz, and the reef platform required by Daly's hypothesis cannot be recognized in the Funafuti boring.

[18]Skeats, Bull. Mus. Comp. Zool., Harvard, XLII, June, 1903.
[19]Op. cit. pp. 174-175.
[20]Op. cit. pp. 392-420.
[21]Mem. Mus. Comp. Zool., Harvard, XXVIII, pp. 21, 22, 1903.
[22]Daly, Proc. Amer. Acad. Sci., ll, p. 247, 1915.

Prof. Daly's opinion, after an examination of the report and of a duplicate set of core material, that below 46 meters the bore continued in talus material to the bottom, appears to the writer to be in conflict with the published facts and with the views of the experts who examined the material. It would involve a sharp, unconformable break at 46 meters below the surface between the coral reef rock and the underlying talus. The evidence on the contrary shows similar organisms and texture above and below this depth, and the only change noticeable near this depth is a gradual mineralogical change from aragonite to calcite between 100-180 ft.

### CONCLUSIONS

The conclusion is reached, based on the examination of the organisms and textures from the bore and the submarine contours of the island of Funafuti, that the only hypothesis of origin capable of correlating and accounting for all these facts is the subsidence theory of Darwin. Supporting evidence of the shallow-water origin of all the material and therefore subsidence of the land will be brought forward in a succeeding paper dealing with the formation of solomite and its distribution among coral and other limestones.

The conclusions to be drawn from this summarized statement of observations, both positive and negative, seem to the writer to be clear, namely, that the whole material of the atoll from the surface down to the bottom of the core at 1114 ft. 6 inches is essentially homogeneous in origin and organisms, and all of it was formed in shallow water. This necessarily involves either a subsidence of the land to the extent of 1114 ft. or possibly while the bulk of the material was formed during subsidence, a small portion may be due to growth during a post-glacial rise in sea level.

## Sleeping Sickness and Big Game

The report of the Sleeping Sickness Commission of the Royal Society, No. XVI (pp. 221+17 plates+3 maps), which bears date 1915, has only just been distributed. This volume, most of the papers in which have been already published in the Proceedings of the Royal Society, gives an account of the investigations carried on by the commission, under the direction of Sir David Bruce, in Nyasaland in 1912-14. The most important conclusion of the commission was that *Trypanosoma brucei*, the cause of magana in Zululand and other parts of Africa, is identical with *T. rhodesiense*, the trypanosome causing sleeping sickness in man in Nyasaland and Rhodesia. On account of the marked infectivity of the game in the fly country—"and this fact stands out most prominently and without any shadow of doubt"—it is recommended that efforts should be made to diminish the number of wild animals in fly-areas, *e. g.*, by removing all restrictions regarding the pursuit and killing of the game. Removal of infected natives, though they are apparently few and far between, to fly-free areas, and the clearing of the forest around villages so as to keep the fly away, are also useful measures, and the suggestion is made that for purposes of administration it would be well to gather the natives together in fairly large villages. Direct measures for the destruction of the fly are not considered to offer any chance of success, but "when the country becomes opened up, cleared, and settled, the big game will disappear and the tsetse with them."

Major Cuthbert Christy contributes to the *Annals of Trop. Med. and Parasitology* (Vol. XI, No. 3, pp. 279-282) a note on tsetse-flies and fly-belts in Central Africa, in which he expresses the opinion that if, in speaking of wild animals or great game, the antelopes are referred to, he is convinced that they play a quite negligible part, if any, in relation to sleeping sickness in man, and that it is possible to exclude with certainty most of the wild animals, though he considers the pig to be the chief culprit, not only the red river-hog and the wart-hog, but more especially the semi-domesticated pig.—*Nature.*

## Hydrogen Reactions and Catalysts

To the young chemist hydrogen is in the first instance the lightest and, so to say, the most elementary of all the elements, and also the general great reducing agent which, though sluggish at ordinary temperature, becomes exceedingly active at high temperatures and reduces most metals, because it combines with the oxygen of metallic compounds. The physicist makes use of the minimal density of the hydrogen when he wants to prepare his "vacuum tubes"; technically that property is utilized in filling balloons, and at present hydrides processes of hydrogen generation find application—*e. g.*, from hydrides, compounds between metals and hydrogen—which the high expense would make impossible in peace times. In the dye industry and in organic chemistry generally the reducing qualities of hydrogen play a very important part. It is not only that hydrogen acts to remove oxygen by combining with it. In many cases addition products are to be formed, and more hydrogen to be introduced into the molecule. The hardening of oils is one of the most important of these reactions. Both the liquid oils, difficult to purify, often of disagreeable smell and taste and hence of low value, and the solid fats, more easily purified and of higher value, are glycerides, that is, compounds of glycerol (or glycerin) with organic acids. In the oils it is oleic acid or some other "unsaturated" acid; the saturated acids of the fats contain two more molecules of hydrogen. To introduce those molecules of hydrogen and to accelerate the reaction for technical purposes, a catalytic agent is needed.

Two of the papers brought before the Faraday Society in December last dealt with the catalytic acceleration of hydrogen reactions. In the first paper, Dr. E. B. Maxted, of Walsall, described experiments on "The Influence of Carbon Monoxide on the Velocity of the Catalytic Hydrogenation of Olive Oil." The hydrogen to be used in synthetic reactions should be pure and free particularly of carbon monoxide as this acts as a poison to the catalyst which, in the hardening of oils, is generally finely-divided nickel. That condition of purity precludes or limits the use of the hydrogen from water gas. Dr. Maxted finds that the first traces of CO (0.25 per cent in his experiments), have relatively the greatest effect on retarding the hydrogenation. He passed mixtures of pure hydrogen with up to 2 per cent of CO into a vessel containing pure olive oil and continued his experiments for one hour, at the end of which about three-quarters of the oil would be saturated; the dilution of the hydrogen with CO would in itself have an influence which he determined by calculation. The results were hardly new, and his "closed-pocket" method was criticized by Dr. H. C. Greenwood, who said he would have preferred a streaming method and would determine the influence of dilution with the aid of nitrogen or some other inert gas. Dr. R. Lessing further pointed out that Dr. Maxted did not state how he prepared his catalyst—a matter of primary importance—and that the impurities would accumulate in the closed vessel and would accentuate the poisoning effect. Owing to the method employed, it looked moreover as if the hydrogenation would altogether be stopped by a few additional per cent of CO. Using nickel carbonyl as catalyst in his own process Dr. Lessing said he could work with hydrogen containing even 50 per cent of CO, which he passed at 200 degrees C. in a continuous stream over the nickel so that the carbonyl was constantly being formed and decomposed again during the reaction, the high efficiency of the process being probably due to the enormous surface of the reduced nickel atoms which were the real catalyst; a mirror deposit of nickel did not act catalytically, he had observed.

The second paper by Dr. E. B. Ludlam on "The Effect of Hydrogen Chloride on the Nitrogen-Hydrogen Equilibrium" concerned the synthetic ammonia process of Haber. It will be remembered that it took Haber and his collaborators—among whom Dr. Greenwood was for a time—years to work out the equilibrium conditions existing between nitrogen, hydrogen and ammonia at temperatures between 500 degrees and 1,000 degrees C. and at pressures of several hundred atmospheres, although it had long been known that nitrogen and hydrogen would combine to a certain, very small extent. The success of the process depends upon the high pressure, the purity of the materials and the efficacy of the catalysts of which a large variety were tried by Haber. At low temperatures the reaction is exceedingly slow; the rate is accelerated by raising the temperature and pressure, but the equilibrium concentration of the ammonia is then very small, and unless the ammonia can be removed from the active zone, it is decomposed again, and the final ammonia yield is low. Now Deville had observed that ammonia was far more stable in the presence of anhydrous hydrochloric acid than when alone. It occurred to Dr. Ludlam to test this statement which, if confirmed, promised to furnish an efficient Haber-Deville process, dispensing with the necessity of high pressures. Ammonia and hydrochloric acid readily combine to ammonium chloride (sal ammoniac), but this salt is known to dissociate on heating again. Strangely enough most of these dissociation experiments do not appear to have been pushed above 300 degrees C., and Deville's statement, that the chloride was only dissociated to the amount of 14 per cent at 920 degrees C., had never been properly revised. In fact, as Professor Alexander Smith (American Chemical Society) pointed out in 1915, very little is known about the dissociation of saturated vapors in general, although those data would have the greatest chemical significance; Smith found that the dissociation of ammonium chloride began to decrease above 300 degrees, did not exceed 67 per cent at 330 degrees, and was probably never complete.

Ludlam first tested Deville's statement in various ways. Passing ammonium chloride vapor forward and backward through quartz tubes charged with pieces of broken quartz, he found that there was little decomposition of the chloride below 900 degrees, but considerable decomposition at about 1,000 degrees. Thus Deville was only partly confirmed. Ludlam then attempted to stabilize the ammonia formation by adding anhydrous gaseous hydrogen chloride to the hydrogen and nitrogen. The gasses were passed through a tube, an axial wire or rod in which was electrically heated. With platinum wires, and with carbon rods, impregnated or not with alkali chlorides or coated with copper, no increase of the ammonia yield was obtained. When a wire spiral of iron—one of Haber's best catalysts—was used, wrapped round a quartz rod, while fumes were observed, and the dull red wire turned bright red; but the fumes were due to ferrous chloride, and not to ammonia. After further unsuccessful experiments the equilibrium was approached from the other side, that is, the decomposition of ammonium chloride in the presence of nitrogen, hydrogen and hydrogen chloride was studied. This was done in a quartz bulb which was heated in a tubular electric furnace. When heated alone for five hours to 950 degrees, without a catalyst, the ammonium chloride remained practically undecomposed, and Deville was thus supported; but when the temperature was raised to 1,100 degrees a decomposition of 10 per cent of the salt was observed. When gold (melting-point 1,063 degrees), or silver (melting-point 961 degrees), were added as catalysts, and the temperature was kept near these melting points for several hours, the decomposition reached 32 per cent; a silver mirror, which turned grey during the heating, caused hardly any decomposition. With catalysts of copper and especially of iron (at 750 degrees), the decomposition was nearly complete, but the resulting gases were hydrogen and nitrogen, and contained hardly any ammonia. The iron experiments were modified and performed at lower temperatures (500 degrees), asbestos being impregnated with ferrous chloride which was then reduced to metallic iron to act as catalyst in a mixture of nitrogen and dry hydrogen chloride. More ammonia was obtained in these experiments; but the formation of ferrous chloride was a disturbing factor, and the doubtfully promising research was discontinued. Dr. Ludlam is not in a position to resume the experiments, and it does not look as if the use of hydrogen chloride would lead to a process that might rival the Haber process. Yet further investigations and in general further determination of accepted facts and "constants" appear desirable.—*Engineering.*

## The Track of a Particle from Radium

DURING the last few years a number of experimenters, including Reinganum, Walmsley and Makower, Miehl, Mayer, Sahni, Kinoshita and Ikeuti, have published excellent photographs showing the tracks of individual $a$ particles from radium which strike the photographic plate at glancing incidence. In a recent paper Kinoshita and Ikeuti (*Journ. Coll. Sci.*, Imper. Univ., Tokio, November 20, 1917), sum up our information on this interesting subject, and show a number of such photographs. Special methods were used to obtain very small radiating nuclei, so that the radial tracks of the expelled $a$ particles show up clearly. The track of an $a$ particle (magnification 500 to 1,500) is not continuous, but marked by a number of developed grains from ten to twenty in number, depending on the velocity of the particle. An estimate is given of the diameter of these grains, with a discussion of the general theory of their formation and of the action of $a$ rays on a photographic plate.—*Nature.*

## Cadmium for Rustproofing

IT is claimed that cadmium in combination with copper or zinc is a valiable material for preventing cast iron, wrought iron and steel from rusting. The iron or steel may be covered with a coating in the following manner: Double salts of cadmium and copper cyanide or of cadmium and zinc cyanide are dissolved in water, and the iron or steel objects electroplated in this solution; in a few minutes the objects will be covered with a coating of cadmium zinc or cadmium copper. The coating is silver-colored and permanently protects against rust. The double-salt solution may be made by dissolving one part of cadmium hydroxide and one part of copper oxide in 100 parts of water. Cyanide of soda or of potassium is added to the solution. If zinc instead of copper is wanted in the coating, zinc oxide is used instead of the copper oxide. Objects to be coated are first cleaned in a diluted acid solution, and are then placed as the negative pole in the solution. For the positive pole a zinc or copper plate is used. Five amperes for each square foot of area to be covered, with a pressure of four volts, is claimed to have been found satisfactory.—*Machinery.*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